

THE MATHEMATICAL GAZETTE

EDITED BY

W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF

F. S. MACAULAY, M.A., D.Sc

AND

PROF. E. T. WHITTAKER, Sc.D., F.R.S.

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Intending members are requested to communicate with one of the Secretaries. The subscription to the Association is 15s. per annum, and is due on Jan. 1st. It includes the subscription to "*The Mathematical Gazette*."

Change of Address should be notified to a Secretary. If Copies of the "*Gazette*" fall for lack of such notification to reach a member, duplicate copies can be supplied only at the published price.

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VOL. XIII.

MARCH, 1926.

No. 181.

The Mathematical Association.

President—Professor G. H. HARDY, D.Sc., F.R.S.

THE Annual Meeting of the Mathematical Association was held at the London Day Training College, Southampton Row, London, W.C. 1, on Monday, 4th January, 1926, at 5.30 p.m., and Tuesday, 5th January, 1926, at 10 a.m. and 2.30 p.m. Professor G. H. Hardy was in the chair.

MONDAY EVENING, 5.30 p.m.

- (1) "Some Problems of Atomic Structure," by Professor E. N. da C. Andrade, D.Sc., Ph.D.

TUESDAY MORNING, 10 a.m.

BUSINESS.

- (2) The Report of the Council for the year 1925 was distributed and taken as read :

REPORT OF THE COUNCIL FOR THE YEAR 1925.

DURING the year 1925, 86 new members of the Association have been admitted, and the number of members now on the Roll is 1042. Of these, 8 are honorary members, 54 are life members by composition, 23 are life members under the old rule, and 957 are ordinary members. The number of Associates remains about 500.

The Council regret to have to record the deaths of Mr. Walter W. Rouse Ball, Fellow of Trinity College, Cambridge, and of University College, London ; Mr. Bertram Bennett, Headmaster of Montpelier School, Paignton, who had been a member of the Association since 1905 ; Mr. William L. E. Bennett, of Dulwich College ; Mr. John M.

Dyer, formerly of Eton College, and a member of this Council for six years; Mr. Percy J. Harding, Honorary Lecturer of Bedford College in the University of London, whose membership of the Association extended over a period of 52 years; the Rev. Francis O. Lane, of King Edward's School, New Street, Birmingham; Mr. William G. Oldland, of Cheltenham Grammar School; Miss M. E. Rickett, formerly Vice-Principal of Newnham College, Cambridge; Mr. K. ff. Swanwick, of the University of Queensland; and the Rev. H. C. Watson, formerly of Clifton College, who joined the Association in 1872 and held the office of Honorary Treasurer from 1878 to 1890.

An obituary notice of Mr. W. W. Rouse Ball, with a portrait, appeared in the October number of the *Gazette*, and obituary notices of Mr. J. M. Dyer and the Rev. H. C. Watson in the March and October numbers respectively.

The General Teaching Committee, the Boys' Schools Committee and the Girls' Schools Committee have recently been re-constituted in accordance with the scheme, and the new Committees will formally enter upon their period of office after the Annual Meeting. The period of office of the retiring Committees was extended from two to three years, and the Council desire to offer to all the members of those Committees, elected and co-opted, the grateful thanks of the Association for all the good work they have done during the past three years in furthering the objects of the Association.

During the past year the Committees have prepared a *Report on the Teaching of Mathematics to Evening Technical Students*, and also a *List of Books* suitable for a School Library as authoritative books of reference. It is hoped that copies of the former will have been in the possession of members before the date of the Annual Meeting, and that it will provide material for a discussion. A memorandum on the teaching of mathematics in Elementary and Junior Technical Schools was sent by request to a consultative Committee of the Board of Education.

The Girls' Schools Committee has prepared a Library list, which was issued with the October number of the *Gazette*; and also a memorandum on the causes which hinder the teaching of mathematics to girls. This memorandum was published in the *Times Literary Supplement* on 21st November, 1925, and will appear in the January (1926) number of the *Gazette*.

As the 200th anniversary of the death of Sir Isaac Newton will fall on the 20th of March, 1927, it is proposed that the event be commemorated in the proceedings of the next Annual Meeting (1927) and in a special number of the *Mathematical Gazette*.

Professor G. H. Hardy retires at this meeting from the office of President, and the Council desire to express their deep sense of the value of the services which he has rendered to the Association during his two years of office.

The Council have the pleasure of nominating Dr. M. J. M. Hill, M.A., LL.D., Sc.D., F.R.S., Emeritus Professor of Mathematics in the University of London and Vice-Chancellor, to be President of the Association for the years 1926 and 1927, and Professor G. H. Hardy to be a Vice-President.

The Council desire to express their high appreciation of the services rendered to the Association by Professor S. Brodetsky, Ph.D., and Dr. W. F. Sheppard during their tenure of office as members of the Council since 1919 and 1920 respectively, and also to acknowledge the indebtedness of the Association to Mr. W. J. Greenstreet for the continuance of his admirable work as Editor of the *Mathematical Gazette*.

By adoption of the Report, Prof. M. J. M. Hill was elected President for the years 1926 and 1927, and Prof. G. H. Hardy was elected a Vice-President.

(3) The Treasurer's Report for the year 1925 was adopted.

(4) The Election of Officers and Members of the Council for the year 1926.

The two places on the Council, vacant by the retirement (in their turn) of Prof. S. Brodetsky and Dr. W. F. Sheppard, were filled by the election of Miss M. A. Hooke and Prof. H. T. H. Piaggio.

(5) The Council proposed that Rule VII be altered as follows : that the words in brackets below be omitted, and the words in italics be added :

Any Member of the Association may nominate Members for election to the Council [either by writing to one of the Secretaries, or by proposing them at the Annual General Meeting]. *Such nominations must reach one of the Secretaries not later than the 25th day of October preceding the Annual General Meeting.*

This was agreed to, with the addition of the words : "These nominations may be supplemented by the Council at its Autumn Meeting."

(6) A paper on "Modern Theories of Integration" was given by E. Carey Francis, M.A. (Fellow and Lecturer of Peterhouse, Cambridge).

The integral defined as the inverse of a differential co-efficient ; failure of this definition. Another method of approach ; the Riemann integral ; integration of a continuous function. Extension ; measure ; the Lebesgue integral ; integration of a bounded function. Further extensions.

(7) Professor C. Spearman, Ph.D., F.R.S., delivered an address on "Measurement of Intelligence."

Prevalent claim to measure intelligence by mental tests. Unexpected difficulties that have arisen ; equivocality of the word ;

historical explanation. Proposed definitions: adaptability to new situations; capacity to learn; neogenetic power. The three doctrines: "monarchic," "oligarchic" and "anarchic." Theory that tests average or sample ability. Theory of Two Factors; a discovery in tables of correlations; deduction of g and s ; nature of g .

- (8) A discussion on "The Proper Function of the *Mathematical Gazette*" was opened by E. R. Brown, B.A.

TUESDAY AFTERNOON, 2.30 p.m.

- (9) The President delivered his Presidential Address, taking as his subject: "The Case against the Mathematical Tripos."
- (10) A discussion on the "Report on the Teaching of Mathematics to Evening Technical Students" was opened by Prof. H. T. H. Piaggio, D.Sc.

The proceedings were brought to a close with the usual votes of thanks.

THIRD ANNUAL REPORT OF QUEENSLAND MATHEMATICAL SOCIETY, 1924-25.

ON Friday evening, 28th March, 1924, at the University, the Third Annual General Meeting of the Society was held. The Annual Report and Balance Sheet were presented and adopted. These showed that the Society was in a satisfactory condition.

An election of officers took place, and then Professor Priestley gave his presidential address on the subject "The History of Newton's Theory of Gravitation."

Three general meetings were held during the year. On Wednesday evening, 21st May, at the Boys' Grammar School, Gregory Terrace, Mr. J. P. McCarthy, M.A., opened a discussion on the Report of the Teaching of Geometry which had been received from the Mathematical Association, London. Many of the members present entered into the discussion.

The second meeting was held at 7 Inns of Court, Adelaide Street, on Friday evening, 1st August, when Mr. K. ff. Swanwick, B.A., submitted for discussion a suggestion, with illustrations, of a method of representing geometrically in three dimensions, points and qualities which in two dimensions are imaginary.

The third meeting of the year was also held at 7 Inns of Court, the speaker being Professor H. J. Priestley, M.A., and the subject "The Geometrical Ideas underlying the Theory of Relativity."

The total number of members of our Society for the year was twenty-two—eleven being full members of the Mathematical Association, this being an increase of two on the previous year's figures. The number of associate members has decreased from sixteen in the previous year to eleven in the year that we have just completed.

The finances of the Society are in a much better state, the balance-sheet showing a credit balance of £1 18s. 11d.—the best to date.

Associate members of the Society have received copies of the *Mathematical Gazette*, as they have come to hand. We are still reserving two copies of the *Gazette* for the use of associate members.

The year has been a successful one, but there must be still a considerable number of persons interested in the teaching of mathematics who are not yet within our ranks.

CARDIFF BRANCH.

Papers read Session 1924-5.

1924.

- Oct. 20. Prof. G. H. Livens : Maxwell's Theory and Electric Waves.
- Nov. 3. Dr. D. G. Taylor : Non-Euclidean Geometry.
- Nov. 17. Mr. A. A. Pope : The Complex Variable.
- Dec. 1. Dr. Paul Dienes : Modern Aspects of Geometry.

1925.

- Jan. 26. Mr. H. A. Hayden : The Theory of Point-Sets.
- Feb. 9. Mr. V. W. Evans : The Place of Non-Euclidean Geometry in Modern Thought.
- Feb. 23. Prof. A. J. Sutton-Pippard, M.B.E. : The Determination of Stresses in Redundant Frameworks.
- Mar. 9. Miss G. M. Weighell : Maps.

Session 1925-6.

The following officers have been elected :

President : Prof. G. H. Livens.

Vice-Presidents : Prof. A. L. Selby, Dr. D. G. Taylor, Mr. A. H. Pope, Mr. H. A. Hayden, Dr. J. H. Shaxby, Dr. R. T. Dunbar, Mr. V. W. Evans.

Committee : Mr. A. Buxton, Miss S. T. Chapple, Miss V. Gah, Mr. A. C. Best.

Secretary : Mr. H. A. Hayden. *Treasurer* : The President.

Papers read.

1925.

- Oct. 19. Presidential Address (Prof. G. H. Livens) : The Development of Modern Principles of Mechanics.
- Nov. 2. Mr. Arnold Buxton : Nomography.
- Nov. 16. Dr. D. G. Taylor : Modern Developments in Number Theory.
- Nov. 30. Dr. J. H. Shaxby : Crystal Structure.

It is requested that any members of the Association in the *S. Wales* area who are not yet members of the Branch will communicate with H. A. Hayden, Newport Road, Cardiff.

GLEANINGS FAR AND NEAR.

346. Martin "in his third voyage discovered a whole kingdom of philosophers, who govern by the mathematics; with whose admirable schemes and projects he returned to benefit his own dear country; but had the misfortune to find them rejected by the envious Ministers of Queen Anne, and himself sent treacherously away."—Arbuthnot, *Memoirs of Scriblerus*, c. xiii.

The aforesaid great Scriblerus "hath enriched mathematics with many precise and geometrical quadratures of the circle."—*Op. cit.*, c. xiv.

His first proposal was "by a general contribution of all princes, to pierce the first crust or nucleus of this our earth quite through, to the next concentrical sphere. The advantage he proposed from this was to find the parallax of the fixed stars; but chiefly to refute Sir Isaac Newton's theory of gravity, and Mr. Halley's of the variations. The second was, to build two poles to the meridian, with immense lighthouses on the top of them, to supply the defect of nature, and to make the longitude as easy to be calculated as the latitude."

347. To a Professor of Law who remarked of Voltaire's universality that he found him weak in nothing but law, d'Alembert replied: "I find him weak in nothing but geometry."—Chamfort.

SOME PROBLEMS OF ATOMIC STRUCTURE.

BY PROFESSOR E. N. DA C. ANDRADE, D.Sc.

BEFORE proceeding to discuss certain aspects of modern atomic theory, I may perhaps be allowed to say a word as to the object of atomic theories in general. Most of us will, I suppose, agree that the object of physical theories is to set up a conceptual scheme which shall make it easier for us to classify and describe the experimental results in which we are interested, by providing, among other things, criteria which shall enable us to distinguish the essential from the unessential, and a scale of values which shall give us help in selecting for investigation certain problems from the infinite field before us. Every important theory is bound to bring into prominence research on certain groups of phenomena at the expense of research on other groups of phenomena: thus the general result of the electron theory is that in any physical laboratory three men out of four are working with vacua, and we see them staring intently at glass tubes with nothing, or very little, shut up in them. An immense number of beautiful results have come from these researches, exhibiting that order which is the true object and delight of the natural philosopher. A few experiments occasioned the original theory: the theory has provided the incentive for a vast number of further experiments, by suggesting for them a significance which would otherwise be lacking. The problem of the connection between radiation, matter and electricity occupies the thoughts of many of the best brains in physical science. This is a result of the electron theory; and this concentration on one problem inevitably draws attention away from other problems. Relatively little is being done, for instance, on the theory of solids, especially in the great problem of cohesion, not because it lacks interest or importance—it has intrigued natural philosophers since scientific speculation began—but because there is no simple, mentally manageable theory to guide us.

A complete atomic theory, which is, of course, an unattainable ideal, would explain all the phenomena of physics and chemistry—cohesion, compressibility, and crystallisation, as well as spectra and corpuscular scattering. The present theory of atomic structure started with sensational quantitative successes in the domain of optical spectra, and thus directed attention to that field of investigation. Regularities of such interest were speedily discovered that researches in spectra, in which term I include X-ray spectra, are at the present time occupying men of science in practically every physical laboratory in the world. The theory, the so-called quantum theory of spectra, has had great successes, although the attempt to explain certain features of spectra, revealed by the recent refinements of research, has brought it up against grave difficulties. Its mathematical nature; the simple, if revolutionary, character of its fundamental postulates; and the beautiful regularities concealed in the apparently random collection of lines which constitute the spectra themselves, seemed to make certain applications of the quantum theory to problems of atomic structure a suitable subject for this address, and accordingly I shall confine myself to spectral problems, while warning you that this is only one aspect of atomic structure.

As I understand that you are mathematicians rather than physicists, I venture to devote a few moments to putting before you some of the most important experimental facts regarding line spectra. There are, of course, three classes of spectra—continuous spectra, in which all wave-lengths are represented, such as those given out by hot solids, or by the sun, if the Fraunhofer lines be disregarded; band spectra, which appear channeled or fluted to a low dispersion instrument, but which high dispersions show to consist of very close lines which crowd together towards certain heads, producing the fluted effect as a draughtsman might produce it by shading with parallel lines;

and line spectra, which consist in general of a large number of lines arranged without any law obvious to casual inspection. Of the band spectra I will say nothing: their explanation, in which some progress has been made, constitutes a problem of atomic structure which is too complicated to be discussed in a few moments. I will content myself with saying that band spectra are always due to molecules, and not atoms, and make that my excuse for the neglect. As regards the continuous spectrum, I should merely like to draw your attention to one little paradox in passing: it was investigation of the continuous spectrum which proved to be the downfall of the theory of light which postulated a continuous structure for radiation. You might suppose that the discontinuous nature of series spectra, where we have a line representing one wave-length,* then nothing, and then another line, would, rather than the continuous spectrum, have led to a theory that radiant energy is atomic in structure, and, in fact, this line structure has proved to be particularly amenable to explanation in terms of the quantum theory of radiation. But the quantum theory was put forward by Planck as a result of an attempt to find mathematically a formula to describe the distribution of energy in the continuous spectrum given by a so-called black body, or complete radiator. It can be shown that if radiant energy could be infinitely subdivided, i.e. if we could have a radiation of a given wave-length of as little energy as we like, then all the energy would be in the shortest waves, whereas we know that there is a maximum of energy at a certain wave-length depending on the temperature. Planck found it necessary to make the startling hypothesis that energy is atomic in structure, that, so to speak, radiant energy is emitted and absorbed in parcels. The magnitude of the atom of radiant energy depends upon the wave-length of the radiation considered in a way given by the fundamental formula

$$E = h\nu,$$

where $\nu = \frac{c}{\lambda}$ is the frequency of the radiation and h is a constant, called Planck's constant. The amount E is called a quantum of radiant energy: it is an atom of radiation. h is of dimensions L^2MT^{-1} , which are those of a moment of momentum: a quantity of these dimensions is called an action, and h is often called the quantum of action.

Line spectra are emitted by gases at low pressure, excited by electric discharge or other means, that is, by atoms which on the whole are comparatively far distant from one another, and so undisturbed by one another's fields. Our wide problem is to account for this emission of a collection of isolated wave-lengths by different atoms. (Similarly, if we had never seen a piano, we might be set the problem of determining its nature by listening outside a girls' school. There might be a large number of pianos, tuned in different keys, emitting different spectra, that is, groups of notes, but in each case we should observe that only a discrete number of wave-lengths, singly or in groups, were emitted, that is, unlike the violin, the piano can only give a finite number of separated notes. From this we should, if clever, form conclusions as to the piano having a keyboard with a finite number of keys.) Now, in the case of many line-spectra, the lines emitted by a given kind of atom can be sorted out into series, certain lines being assigned to one series, certain other lines to another spectral series, and so on. The lines of a single series, once it has been disentangled, form what is obviously an ordered structure: the methods by which the lines can be sorted out are not suitable for discussion here. The regularities found are more simply expressed in terms of the frequency of the line than of the wave-length—that is, more simply expressed in terms of $\frac{c}{\lambda}$ than λ . Actually, c being a constant, $\frac{1}{\lambda}$ is

* Strictly speaking, a very narrow group of wave-lengths.

usually adopted by the spectroscopists: it is called the wave-number, and expresses the number of wave-lengths per cm. It is usually denoted by ν . Now it is found, in the case of spectra which have been ordered (and the number is constantly increasing), that the wave-numbers of a line can be expressed as differences of a pair of numbers chosen from a group of numbers peculiar to the spectrum. These numbers are called terms. Thus

$$\nu = T_{n'} - T_n,$$

where $T_{n'}$ and T_n are two terms. The important thing is, of course, that a comparatively small number of terms suffices to express a large number of lines, the same terms being used for many different lines. The numerical value of these terms can be expressed by various empirical formulae, of which I will take the simplest, that of Rydberg. This is

$$T_{n,a} = \frac{R}{(n + \mu_a)^2},$$

where μ_a is a constant and n is a whole number, which takes the value 1, 2, 3, ... consecutively,* giving a sequence of terms. In a given spectrum, there are, in general, four different sequences of terms, corresponding to different values of the constant μ , but the point which I wish to emphasise is that the lines of a given spectral series are given by a formula of the type

$$\nu = R \left\{ \frac{1}{(n' + \mu_a)^2} - \frac{1}{(n + \mu_b)^2} \right\},$$

where the first term remains constant and the second varies in steps for the different lines of the series, n taking successive whole-number values.

The structure of a line spectrum may be illustrated by the example of lithium, shown in Fig. 1, where the different series are shown separated. In the actual spectrum they are, of course, all together in one band.

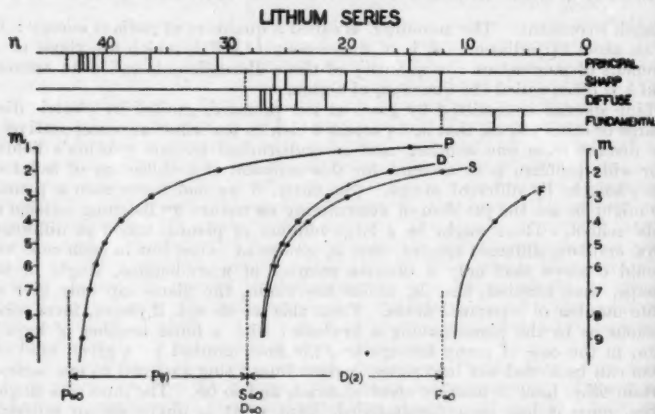


FIG. 1.

The essential point to remember is that the wave-length number of a spectra line is given by the difference of the terms selected from different term sequences. There are limitations imposed upon the pairs of terms which may

* The beginning number may not be 1, but some other whole number; the essential point is that n adopts consecutive whole number values.

be taken together, and these limitations are an important feature of Bohr's quantum theory, to which reference will be made later.

This brief summary of the empirical laws of line spectra must suffice. We now turn to the quantum theory of those spectra. This is based upon the theory of atomic structure which Rutherford put forward in 1911 as the result of his experiments on the scattering of α particles. The atom is assumed to consist of a central positively charged sun, or *nucleus*, in which practically all the mass is concentrated: this is very small, of the order 10^{-12} cm. in diameter. Around the nucleus the various electrons composing the atomic structure circulate as planets, under the influence of the central force of the nucleus, and any action which they may exert on one another. The diameter of the atom, by which we mean the linear dimensions of the normal orbits of the outer electron, is about 10^{-8} cms. Thus, to get an idea of the scale of the atom, for a sodium atom we can take the nucleus as a tennis ball, the nearest electrons in circular orbits of about 100 yards radius, and the diameter defined by the outer electrons as about a mile. The central charge in general we will call Ze : then there will be Z electrons in a neutral atom. Z is called the atomic number: it is, roughly speaking, the number expressing the place of the element when all the elements are arranged in order of their atomic weights.

The wide problem is to connect the motion of the electrons with the spectrum emitted by the given atom. Obviously any problem in which we are confronted by a large number of moving bodies acting upon one another with an inverse square (or any other) law of force is not capable of an exact solution. We must find simplifying rules. Now, on the Maxwellian, or classical, theory of electrodynamics, any system in which electrons are circulating in orbits must give a continuous spectrum, for any acceleration of an electron is, on the classical theory, accompanied by radiation and consequent loss of energy, so that the period of the electron in its orbit would change continuously. The frequency of the radiation is equal to the orbital frequency, so that a circulating electron would give a slightly different wave-length at every revolution. Bohr surmounted this difficulty in a very simple way, by denying that an atom in which electrons are moving steadily in orbits does radiate. This is, of course, a sheer assumption: it has been so successful that we have become used to it, and an assumption to which one is accustomed is as good as true. The lack of radiation must simply be set down to some peculiarity which we do not understand in the interrelation between the atoms of electricity bound in the atom of matter and the atoms of radiation.

Bohr's second assumption is that, of the continuous sequence of states possible on mechanical grounds, certain discrete states are privileged, and have a peculiar stability, these states being determined by certain quantum conditions which equate quantities of the dimensions of an action to be equal to a whole number multiple of Planck's constant, h . Such a privileged state is called a *stationary state*, and the hypothesis of stationary states is the very essence of Bohr's theory. To the sequence of stationary states belong a sequence of energies of the atomic system E_1, E_2, \dots , so that the atomic system in temporary stability can possess only one of a given range of discrete energies.

The third assumption is that an atom in a given stationary state, of energy E_n , say, can pass abruptly and spontaneously to a stationary state of the energy E_m . When such a transition, or, as it is sometimes called, quantum switch, occurs, the atom radiates and the frequency of the radiation ν_r is given by the fundamental equation

$$h\nu_r = E_m - E_n.$$

The loss of energy of the system appears as radiation, and the frequency of the radiation is given by the quantum relationship.

These assumptions have a certain *ad hoc* character: for instance, the third assumption is obviously made to fit the experimental fact that the frequency can be expressed by the difference of two terms. Like all general theories it is a hypothetical generalisation from a small number of facts which has proved successful in describing a much greater number. The justification of the theory is its quantitative success in a certain field, and its general descriptive success in a wider and more inclusive field. In particular, certain experiments on the energy needed to make an atom radiate, which will be mentioned again at the end of the lecture, have given striking confirmation of the assumptions.

To illustrate the significance of these three assumptions, I will consider the simplest possible case, of one electron circulating round a heavy nucleus, which represents the atom of neutral hydrogen. The orbits are, of course, simple Keplerian ellipses, but we will consider the particularly simple case when they are circular. The first assumption tells us that an electron can rotate in a circular or other orbit without radiating: on classical theory if it rotated ω times a second it would emit a radiation of frequency ω . The second assumption is that, for some reasons unknown, of all possible circular orbits, only certain ones have stability, and these orbits must be fixed by a quantum condition. The simplest way of expressing the restricting quantum condition is to say that the angular momentum in the case of a stationary state must have the value $\frac{nh}{2\pi}$, where n is a whole number, or

$$mvr = \frac{nh}{2\pi} \dots\dots\dots (1)$$

Very simple calculation shows that $\omega = \frac{4\pi^2 m Z^2 e^4}{h^3} \cdot \frac{1}{n^3}$, and the total energy

$$= E = -W = -\frac{2\pi^2 m Z^2 e^4}{h^3} \cdot \frac{1}{n^2}$$

if the energy of the atom when the electron is at rest at infinity be taken as zero. The third condition then gives for hydrogen ($Z=1$)

$$r = \frac{2\pi^2 m e^4}{c h^3} \left(\frac{1}{n'^2} - \frac{1}{n^2} \right), \dots\dots\dots (2)$$

where n' and n are whole numbers. This gives us a single sequence of terms, $C \cdot \frac{1}{n^2}$, instead of the four sequences to which we have referred in the case of the general atom: if we put $\mu_1 = \mu_2 = \dots = 0$ in the various sequences which Rydberg's formula would represent for the general atom, we get this result.

If we put $n' = 2$ and n successively 3, 4, 5, ..., we get a series of wave-numbers which agree exactly with the measured wave-numbers of the well-known Balmer series of hydrogen, provided that constant C has the value of Rydberg's constant R , i.e. 109,678. The striking thing is, not that we have derived Balmer's formula $\nu = R \left(\frac{1}{n'^2} - \frac{1}{n^2} \right)$, which we could scarcely fail to do, since all our assumptions were designed to produce it, but that if we put the experimentally found value of e , m and h into formula (2) we get the correct value for the constant. This was the first important confirmation of Bohr's theory. Putting $n' = 1$, and letting n vary, we have another series of lines: the so-called Lyman series; putting $n' = 3$, we get the Paschen series. (See Fig. 2.)

Another very striking confirmation was furnished by the spectrum of ionised helium, to which I must refer very briefly. Neutral helium consists of a nucleus with a positive charge of two units, and two circulating electrons.

It gives a fairly complicated spectrum, which still, however, gives as much trouble as any to the theoretical spectroscopist. Suppose, however, that the helium atom is ionised, or positively charged, by the permanent removal of an electron: we shall then have an atom just like hydrogen, except that the nuclear charge is two units instead of one. The result of this is, that the con-

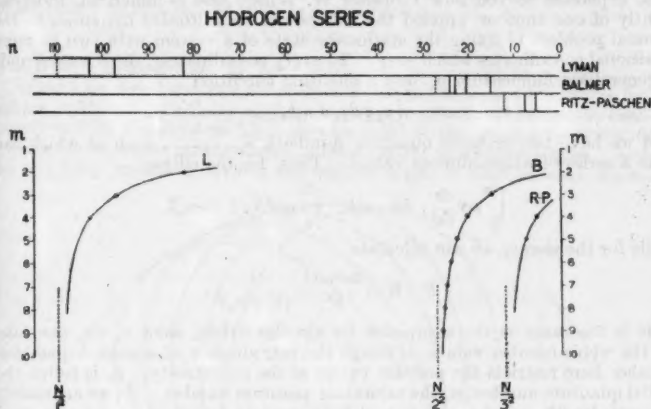


FIG. 2.

stant in formula (2) is multiplied by $2^2 = 4$, or we have for the expected spectrum of ionised helium

$$\nu = 4R \left(\frac{1}{n^2} - \frac{1}{n'^2} \right).$$

The value of R will not, however, be quite the same as in the hydrogen formula for the following reason. In deducing that formula we assumed that the nucleus was infinitely heavy compared to the electron. Actually, it is about 1840 times as heavy as the electron in the case of hydrogen, and $4 \times 1840 = 7360$ times as heavy in the case of helium. The system rotates about its centre of mass, very close to, but not in, the centre of the nucleus. When this is taken into account, the value of R comes out slightly different for hydrogen and ionised helium, and instead of the lines of ionised helium coming exactly on every other line of the Balmer series of hydrogen, they are slightly displaced from this position. Such lines, which were originally believed to be hydrogen lines, have been very carefully measured, and with the help of Bohr's formulae, a value of e/m can be derived from this displacement which agrees excellently with that measured by well-known experimental methods.

I may mention here that, in general, ionised atoms give spectra which are quite different from those given by the neutral atoms, $4R$ replacing R in the formula, and the value of the other constants being also modified. The doubly ionised atoms can also be detected, with constant $9R$, and higher degrees of ionisation have also been found. This is mentioned in passing to explain why the spectrum of the neutral atom is often specified.*

The question of circular orbits is comparatively simple, and the restricting quantum condition (1) is simple. The formulation of the quantum condition

* The general character of the modification caused by ionisation can be expressed in a very interesting simple rule due to Kossel and Sommerfeld, but cannot be here discussed for want of space.

in the general case is one of the great problems of atomic structure, that is, how to define mathematically which states are stationary states. Let us, still taking the hydrogen atom, consider the elliptic orbit. Here there are two co-ordinates which vary with time, r and θ , instead of only one, θ , as in case of the circle. Professor Nicholson, when he addressed you four years ago, explained to you how Professor W. Wilson and Sommerfeld, independently of one another, applied the methods of Hamiltonian dynamics to the general problem of fixing the stationary state of a system with two or more positional co-ordinates which vary. To every co-ordinate q_i there corresponds a generalised momentum p_i , and a quantum condition

$$\oint p_i \cdot dq_i = n_i h,$$

and we have two or more quantum numbers, n_1, n_2, \dots , each of which can take a series of whole-number values. Thus, for the ellipse

$$\int_0^{2\pi} m \dot{r} \frac{dr}{d\theta} \cdot d\theta = n_r h, \quad \int_0^{2\pi} m r^2 \dot{\theta} \cdot d\theta = n_\theta h,$$

while for the energy we can calculate

$$-E = W = \frac{2\pi^2 m e^4}{h^2} \cdot \frac{1}{(n_r + n_\theta)^2}.$$

This is the same as the expression for circular orbits, since $n_r + n_\theta$ can take all the whole-number values, although the introduction of a second quantum number here restricts the possible values of the eccentricity. n_r is called the radial quantum number, n_θ the azimuthal quantum number. As we are mainly concerned with energies, however, it is convenient to consider $n = n_r + n_\theta$, and to call n the total quantum number. We can specify n and n_θ , instead of n_θ and n_r , and n gives the energy directly in the case of the simple Kepler ellipse.

However, there is another way of regarding the problem of fixing the stationary states, a way favoured by Bohr. While it is not very simple mathematically, nor suited for detailed exposition here, yet the general principles which govern it can perhaps be made clear in a few words. Bohr's so-called Correspondence Principle shows that, although on the quantum theory the frequency of the radiation is not, as on the classical theory, equal to the frequency of the motion of the electron in its orbit, yet there is a certain correspondence between the motion in the orbit and the radiation emitted. An elementary calculation shows, for instance, that the frequency of the radiation given by the hydrogen atom already considered is, in the limit where $(n' - n)$ is very small compared to n' , equal to that of the rotation of the electron. Bohr is led to attribute a particular significance to the character of the periodic orbital motion of the electron. In the simple Keplerian ellipse there is only one frequency which need be specified: the electron executes, say, ω_1 rotations in unit time. If we consider the x component of the motion we can by Fourier's theorem express it in terms of ω alone; thus

$$x = \sum C \cos 2\pi(\tau\omega_1 t + \gamma) \dots\dots\dots (3)$$

ω_1 being the so-called fundamental frequency. Suppose, however, that the ellipse be disturbed in a certain way: suppose, for simplicity, that the perihelion rotates with a frequency ω_2 not commensurable with ω_1 , as shown in Fig. 3. We then require both ω_1 and ω_2 to express the variation of x with time; thus

$$x = \sum \sum C_{\tau_1, \tau_2} \cos [2\pi(\tau_1\omega_1 + \tau_2\omega_2)t + \gamma_{\tau_1, \tau_2}] \dots\dots\dots (4)$$

We call this a motion of two degrees of periodicity. In general, we may have more degrees of periodicity: for instance, if the plane of the orbit rotate, we have a third.

Now Bohr connects his quantising conditions with these periodicities, and, in particular, he introduces one quantum number (which, of course, takes a series of whole-number values) for each degree of periodicity. He considers certain quantities J_1, J_2, \dots , which are actions, and connected with the angle variables of formula (4), and puts $J_1 = n_1 h, J_2 = n_2 h, \dots$. There is one J for each different ω , and there is a condition

$$\delta E = \sum \omega \cdot \delta J,$$

which helps to fix the energy, but we can only refer to this in passing.

What I hope you can now see at once is that in the case of the simple Kepler ellipse, with one periodicity ω , there is only one quantum number, so that the result will be the same as for a circular orbit of the same periodicity, i.e. a circular orbit whose radius equals the semi-major axis of the ellipse. As soon as anything occurs to disturb the elliptic orbit, so that imposed upon the

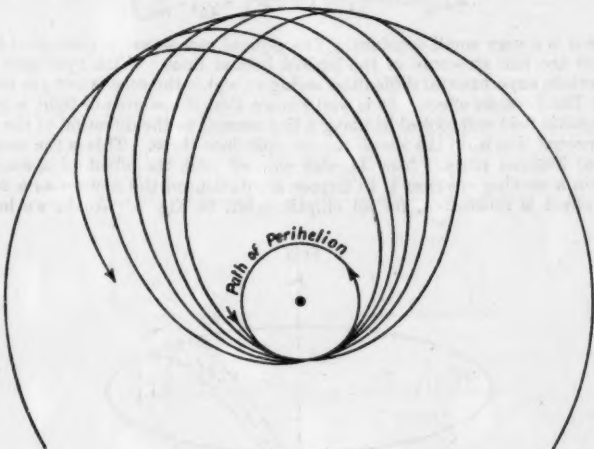


Fig. 3.

general motion in a nearly closed orbit there is another periodic motion, a second quantum number comes in, and the energy depends upon two quantum numbers, each of which can vary, although, if the disturbance be small, it is determined to a first approximation by the quantum number corresponding to the undisturbed orbit, which is called the principal quantum number. The result of such a secular disturbance is, then, that instead of getting a single energy value corresponding to a fixed value of the total quantum number, we get several near values corresponding to different values of the second, or azimuthal, quantum number. We should, therefore, connect any splitting up of a spectral line observed in the laboratory with a disturbance of the motion of the electron in its orbit.

There are three chief effects which have been thus explained by the quantum theory, with excellent quantitative agreement in the simple case of the hydrogen-like atom, i.e. a nucleus with one circulating electron. They are: (a) the so-called fine structure of the Balmer lines of hydrogen, and the lines of ionised helium. These lines, under very high resolution, prove to have a complicated structure, consisting of several components. I can show you a picture of the effect for two lines of ionised helium, but you must remember

that the resolution is very high, for the total separation which you can see is only about a twentieth of that of the two *D* lines of sodium. (A slide was here shown.) Now this fine structure has been worked out in detail on the following lines: owing to the change of mass with velocity which is an essential feature of the theory of relativity, the mass of the planetary body is greater at perihelion than at aphelion. This change of mass is mathematically equivalent to a departure of the law of force from the inverse square, and leads to a slow motion of perihelion, exemplified in astronomy by the motion of the perihelion of Mercury. In the case of the hydrogen-like atom the perihelion also rotates, as shown in an exaggerated way in Fig. 3. Actually for hydrogen the perihelion only rotates once for forty thousand revolutions of the electron in its orbit. Thus we have here a second degree of periodicity and a second quantum number, actually leading to the formula

$$E_{n,n_a} = -\frac{RZ^2hc}{n^2} \left\{ 1 + a^2 Z^2 \left(-\frac{3}{4n^2} + \frac{1}{nn_a} \right) \right\},$$

where *a* is a very small constant. The general agreement is very good in the case of the fine structure of the ionised helium lines. With hydrogen there are certain experimental difficulties owing to which the case is not yet settled.

(b) The Zeeman effect. It is well known that if a source of light is put in a magnetic field and looked at along a line normal to the direction of the field, the spectral line is, in the simplest case, split into three. This is the so-called normal Zeeman effect. Now Larmor showed that the effect of a magnetic field on a moving electron is to impose a rotation on the motion as a whole. This effect is illustrated, for an elliptic orbit, in Fig. 4. Again we have a

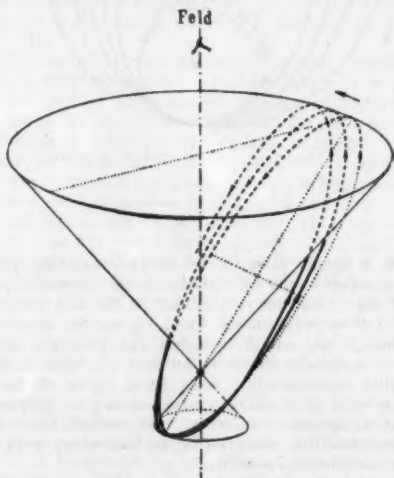


FIG. 4.

second periodicity introduced. The calculation of the normal Zeeman effect on quantum lines leads to perfectly satisfactory results, but it may be stated that the same results had been reached on classical lines many years ago by Lorentz.

(c) The Stark effect. In 1913, Stark, overcoming certain grave experimental difficulties, discovered that if atoms emitting light be placed in a powerful electric field the single line is split up into many components, as shown for the hydrogen line in the slide. The effect of a uniform parallel field of electric force on the orbit is very complicated, since it is not a central force, like the nuclear attraction. The orbit goes through a cycle of changes of shape as well as position, but in the straightforward case there is only one new periodicity introduced. How well calculation agrees with theory, both as regards intensity and position of the components, is illustrated by Fig. 5.

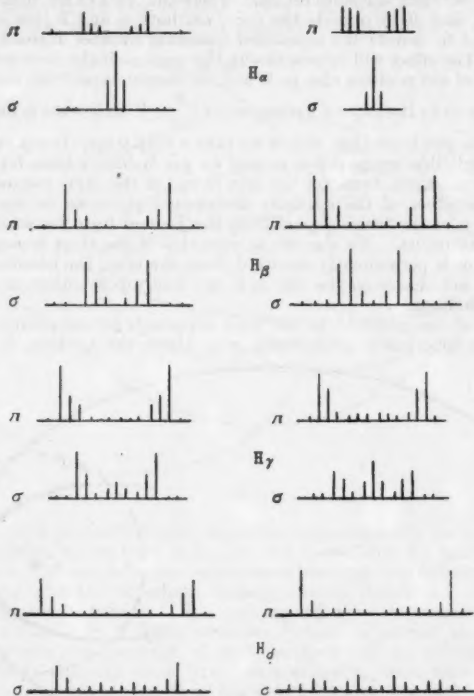


FIG. 5.

The calculated pattern is on the left, the observed pattern on the right. The length of the lines indicate the intensities of the components. The explanation of the Stark effect is one of the triumphs of the quantum theory, since classical electromagnetic theory had proved quite unable to predict or calculate the nature of the effect.

So far we have been talking about the hydrogen-like atom, and we see that with such a simple atom the quantum theory of spectra has been strikingly successful. Let us now take a hasty glance at the problem of the general atom. We know that the optical spectrum of an atom is changed in character by chemical combination, or by very intense electric discharges as compared with a milder one, and these facts and others lead us to suppose that the

electron whose changes of orbit are responsible for optical spectra moves, for most of the time at any rate, in the outer part of the atom. We suppose, anyhow in the simpler cases, that one electron, occupying a position in the outer part of the atomic structure, can occupy a variety of possible orbits, besides its normal one, these orbits lying for the greater part of their path, at any rate, right outside the atom. We call this particular electron the optical, or series, electron, and the rest of the atom (nucleus + all electrons but one) the core. The various possible orbits of the series electron correspond to various possible stationary states of the atom as a whole.

Let us first consider the simplest case, where the orbit of the optical electron is very large, and right outside the core, i.e. both n and k (the symbol k is generally used to denote the azimuthal quantum number instead of n_z) are very large. The effect will be practically the same as if the core were replaced by a nucleus of net positive charge 1, and we should expect the same formula

for the energy as in the case of hydrogen, i.e. $\frac{R}{n^2}$. This is what is found experi-

mentally as, to put it another way, if we take n very large, then μ in Rydberg's formula is negligible compared to n , and we get Balmer's form for the higher terms. This accounts, then, for the occurrence of the same constant R in all spectra. The effect of the spatially distributed electrons in the core is to make the form of the terms depart from the Balmer form for small values of n , i.e. for small orbits. We also see at once that if the atom is singly ionised, i.e. an electron is permanently removed from the core, the constant becomes $4R$, since the net charge on the core is 2, as already pointed out in connection with ionised helium.

The effect of the electrons in the core, supposed, for simplicity, to be distributed in a spherically symmetrical way about the nucleus, is to disturb

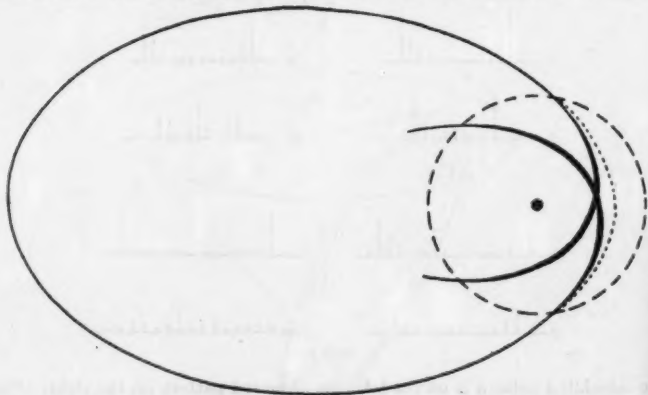


FIG. 6.

the inverse square nature of the central forces for electrons approaching very near, or penetrating, the core in the course of their journey. The effect of any departure from the inverse square law is to cause the perihelion to rotate, and so to introduce a new periodicity, and a second quantum number. The general type of penetrating orbit is shown in Fig. 6: there is an external loop, practically elliptical, and an inner loop, showing rapid rotation of perihelion. Now the principal quantum number is that which gives the energy in the limit when the disturbance is reduced to zero, so that we have a simple ellipse

with one periodicity. In that limit all ellipses of the same major axis have the same energy, so that, for example, the four ellipses represented in Fig. 7, all of principal quantum number 4, but having respectively azimuthal quantum number 1, 2, 3 and 4, all have practically the same energy when the inverse square law holds very closely. This is the hydrogen case, where all four are energetically equivalent. When, however, there is a disturbing field near the nucleus the elliptical nature of the orbit is destroyed in that region. We see at once that the disturbance must be the greatest for the ellipses of highest eccentricity, i.e. smallest k , and that it will be different for each different k , being very slight in the case of the circular orbit, where $k=n$. Hence if we fix $k=1$, and let n vary, we get one sequence of terms; if we fix $k=2$, and let

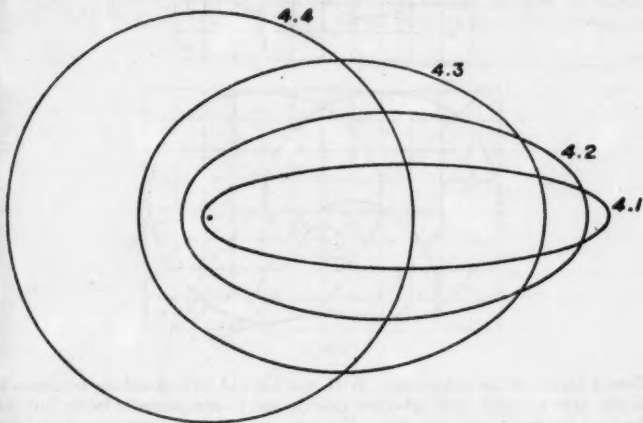


FIG. 7.

n vary, we get a second different sequence, approximating more closely to a Balmer sequence, and so on. It is thus easy to see how the presence of other electrons causes the single Balmer sequence to break up into different sequences, in accordance with the experimental facts already discussed. You may ask why k should not be taken $=5, 6, \dots$, giving still other sequences beyond the four. Theoretically it is quite possible, but consideration shows that for technical reasons the presence of such terms would be difficult to detect experimentally, although they have, exceptionally, been traced in certain cases. It may be added that Bohr has shown how to calculate on these lines both the general Rydberg form of term, and another form of term, due to Ritz, approximating more closely to experiment.

A certain amount of quantitative work has been done on spectra given by an atom consisting of a core and a single privileged electron, such as we suppose the neutral sodium atom, or the ionised magnesium atom to be. This work has taken the direction of trying to calculate a spherically symmetrical disturbing field which shall give the energies of various orbits in agreement with the term values derived from experiment. Very fair success has been recorded by Hartree, Fues and others with sodium atoms along these lines: considering the complex motion of the electrons in the core it is rather surprising that this simple hypothesis of a static spherically symmetrical core should give approximately correct results. I show you the shape of some of the orbits calculated on these lines. (Fig. 8.)

A disturbed plane central orbit can, as we have seen, be described, restricted to a given size and shape, that is, by two quantum numbers. With these two quantum numbers we can, theoretically, obtain a formula for the spectrum term of the type which has been experimentally found, and account, as we have seen, for the existence of different types of series—sharp, principal, diffuse, fundamental—one quantum number being constant for the given type of term, while different values of the other quantum number give the

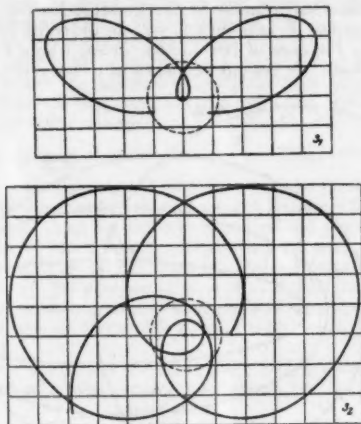


FIG. 8.

different terms of the sequence. With the help of two quantum numbers we can also give a rough, half-intuitive description of the periodic table, but that is beyond our scope this evening. We can also get a quantitative description of a certain very restricted class of spectra. Our problem is, however, to give a quantitative description of the line spectra in general, and that brings us to a new stage in our investigations, which requires a few more words as to experimental fact.

Spectra are actually not so simple as described earlier in the lecture. A given series, say the principal series, does not in general consist of single lines spaced out in accordance with the laws already sketched, but of groups of lines so spaced out: that is, instead of a single line we have a multiple line, a doublet, such as the famous *D* line of sodium (which is the first number of the principal series of sodium), or a triplet, or, in general, what is called a multiplet. The triplet system of magnesium will provide an example. (A slide of the triplet system was here shown.) A spectrum of this kind contains the usual four series of lines, all triple (or actually, in some cases, sextuple, but this complication may be neglected: it is comparatively simply explained). It also contains series of single lines, such as we have considered hitherto: that is, it contains what is known as a singlet system and a triplet system. The type of system depends upon the chemical nature of the atom, that is, the column of the periodic table to which it belongs: thus neutral magnesium, calcium, strontium and barium have similar systems. Many interesting regularities are known about these systems, such as that a neutral atom has systems of either odd or even multiplicity, but not both, but the only fact to which I invite your attention is the existence of these multiple lines.

Two quantum numbers are obviously insufficient to describe multiple lines, since, when they both have values allotted, they merely fix one term of one sequence. We want a third varying number, different values of which shall fix different components of a given term, to give the multiplets. According to Bohr, this means that we must have a third type of periodicity in the orbital motion which can be given as follows. The period in the orbit is one periodicity, the period of rotation of perihelion is a second: if we let the plane of the orbit precess about a fixed axis we have a third. For this to happen we must suppose that the core itself has a moment of momentum, providing a privileged direction in space: core and series electron will then precess so that the total moment of momentum OD , obtained by compounding the angular momentum OC of the core and CD of the electron vectorially, remains fixed in space (Fig. 9). This, then, gives us our third periodicity,

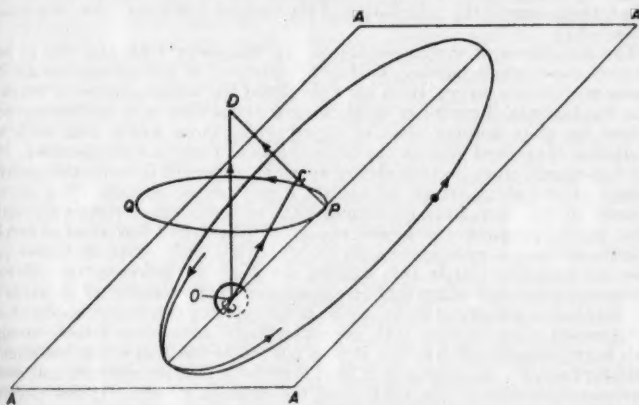


FIG. 9.

and all would at first sight appear well. This, however, is far from being the case.

A discussion of the nature of the difficulties would lead us far afield, but we may glance at a closely-allied problem to that of explaining the multiplet structure, since, after all, my object is to indicate some of the problems of atomic structure, and I have not promised solutions. I refer to the so-called anomalous Zeeman effect. This is the name given to the effect of a magnetic field in splitting up any one of the line components of a multiplet, for with such lines we do not get the simple resolution into three components discussed earlier, which is called the normal Zeeman effect (normal, because simple, and not because frequently met with, for the abnormal effect is the common one), but a resolution into a large number of lines. The anomalous Zeeman effect comprises a large number of types of splitting, the type depending upon the type of the line, whether a principal, sharp, diffuse, or fundamental line, and whether a doublet, triplet, or other multiplet line. Actually it is the nature of the term, rather than the line, which must be considered: that this is so is a fundamental consequence of the very nature of Bohr's theory. Considerations of the anomalous Zeeman effect lead us to attribute half-number values to the quantum numbers, in order to express the experimental result, which is very unsatisfactory. Certain features of the doublet structure of X-ray spectra, and of optical spectra, lead to further difficulties, so that,

when we come to consider the problem of general spectra, it is the general opinion nowadays that the interconnection between the core and the series electron is not governed by mechanical laws. As Bohr says, "Just as no mechanical explanation can be given for the stability of the atom in collision, so we must suppose that already, in the description of the stationary states of the atom, the special part which every electron plays in its interaction with the other electrons is secured in an entirely unmechanical way."

So there we are. But the positive results of the theory are striking enough. Even the most complicated spectra are now being resolved into terms, to which certain whole number (or half-whole-number, alas!) values are empirically allotted. When these values have been allotted to the terms, the frequencies of the lines are given by $h\nu = E_{n'} - E_n$, and the possibility of transitions is given by rules regarding the amount by which these numbers can change in a single transition, or quantum switch, and, in the case of the Zeeman effect, the nature of the polarisation of the components is also given by similar rules.

The hypothesis of stationary states, the frequency rule, and the general discontinuous, whole number, arithmetic character of the mechanism are the features of Bohr's theory which have simplified the whole problem of spectra. The fundamental hypothesis of stationary states has been brilliantly confirmed by quite another class of experiments, those which deal with the excitation of spectral lines by the bombardment of atoms with electrons. On our hypothesis, if an electron strikes an atom, it cannot increase the internal energy of the atom except by certain abrupt steps, corresponding to the transfer of the atom from its normal state to some higher stationary state. That is, if the electron possesses energy of translation less than a certain definite amount, it must make a perfectly elastic collision with the atom, just like one perfectly elastic ball striking another. If, however, the electron possesses energy exceeding that corresponding to the transfer of an atom to the next higher stationary state, say S_1 , it may give up this energy to the atom, and proceed after collision with correspondingly diminished kinetic energy. This is an inelastic collision, but it does not follow the laws of the collision of inelastic balls, for the energy sacrificed must be a definite amount, and not a continuous function of the velocity of the impinging body. If the electron has still greater energy it may raise the atom to a higher stationary state than that next to the normal, say S_2 ; or to other still higher states, S_3, S_4, \dots . Considering the history of atoms after collisions which have put them in the so-called excited state, we can easily see that when they return from the state S_1 to the normal state S , they will give out one spectral line only: there is only one possible type of transition. If, however, the bombarding electrons have had more energy, and raised the atoms to stationary states of more energy than S_1 , we have various methods of return to the normal state. According to the energy of the electrons, we should be able to excite the spectra in bits, one line only, or two lines, or three lines, or all the lines of one series.

Such experiments have actually been carried out, and the spectrum has, in fact, been excited in stages, the necessary energies corresponding exactly to those demanded by Bohr's theory.

I seem to have taken up a great deal of your time, and, looking back, to have told you very little, especially about the difficulties with which the theory is meeting. I am painfully aware of the sketchy nature of this attempt to depict some of the beauties of the new theory, but I must crave your indulgence on the ground that to compress the essentials of contemporary work, where proper perspective is hard to gain, into a brief discourse is a task which might well tax one more competent than myself.

E. N. DA C. ANDRADE

THE CASE AGAINST THE MATHEMATICAL TRIPOS.

(Presidential Address to the Mathematical Association, 1926.)

My address to-day is the result of an informal discussion which arose at our meeting last year after the reading of Mr. Bryon Heywood's paper. You may remember that Mr. Heywood put forward a number of suggestions, with whose general trend I found myself entirely in sympathy, for the improvement of the courses in higher pure mathematics in English universities. He did not criticise one university more than another; but Cambridge is admittedly the centre of English mathematics, so that it is almost inevitable that such suggestions should be considered from the Cambridge standpoint; and that, if my recollections are correct, is what actually happened in the discussion.

My own contribution to the discussion consisted merely in an expression of my feeling that the best thing that could happen to English mathematics, and to Cambridge mathematics in particular, would be that the Mathematical Tripos should be abolished. I stated this on the spur of the moment, but it is my considered opinion, and I propose to defend it at length to-day. And I am particularly anxious that you should understand quite clearly that I mean exactly what I say; that by "abolished" I mean "abolished", and not "reformed"; that if I were prepared to co-operate, as in fact I have co-operated in the past, in "reforming" the Tripos, it would be because I could see no chance of any more revolutionary change; and that my "reforms" would be directed deliberately towards destroying the traditions of the examination and so preparing the way for its extinction.

There are, however, certain possible grounds of misunderstanding which I wish to remove before I attempt to justify my view in detail. The first of these is unimportant and personal, but probably I shall be wise if I refer to it and deal with it explicitly. Our proceedings here do not as a rule attract a great deal of attention, but they are occasionally noticed in the press; and the writer of a well-known column in an evening paper, who was inspired last year to comment on these particular remarks of my own, observed that it was unnecessary to take such iconoclastic proposals seriously, since Cambridge mathematicians were very unlikely to be disturbed by the criticisms of an Oxford man. Perhaps, then, I had better begin by stating that the Mathematical Tripos is an institution of which I have an extensive and intimate knowledge. It is true that I have not taken any part in it during the last six years; but I was a candidate in both parts of it, I took my degree on it, I have examined in it repeatedly under both the old regulations and the new, and, when the old order of merit was abolished in 1910, I was a secretary of the committee which forced this and other changes through a reluctant Senate. I am not then a mere jealous outsider, itching to destroy an institution which I cannot comprehend, but a critic perfectly competent to express an opinion on a subject which I happen to know unusually well.

The second possible misapprehension which I am anxious to remove is decidedly more important. It is possible that some of you may have come here expecting me to deliver a general denunciation of examinations; and if so I am afraid that I shall disappoint you. Denunciation of examinations, like denunciation of lectures, is very popular now among educational reformers, and I wish to say at once that most of what they say, on the one topic and on the other, appears to me to be little better than nonsense. I judge such denunciations, naturally, as a mathematician; and it has always seemed to me that mathematics among all subjects is, up to a point, the subject most obviously adapted to teaching by lecture and to test by examination. If I wish to teach twenty pupils, for example, the exponential theorem, the product theorem for the sine, or any of the standard theorems of analysis or

geometry, it seems to me that by far the best, the simplest, and the most economical course is to assemble them in a lecture-room and explain to them collectively the essentials of the proofs. It seems to me also that, if I wish afterwards to be certain that they have understood me, the obviously sensible way of finding out is to ask them to reproduce the substance of what I said, or to apply the theorems which I proved to simple examples. In short, up to a point, I believe in formal lectures, and I believe also in formal examinations.

There are in fact certain traditional purposes of examinations, purposes for which they always have been used, and for which they seem to me to be the obvious and the appropriate instrument. There are certain qualities of mind which it is often necessary to test, and which can be tested by examination much more simply and more effectively than in any other way. If a teacher wishes to test his pupils' industry, for example, or their capacity to understand something he has told them, something perhaps of no high order of difficulty, but difficult enough to require some little real intelligence and patience for its appreciation, it seems to me that his most reasonable course is to subject them to some sort of examination. Examinations have been used in this manner, from time immemorial, in every civilised country; there are, in England, quite a number of large, elaborately organised, and, so far as I can judge, quite sensibly conducted examinations of this type; and with such examinations I have no sort of quarrel.

There are, however, in England, and, so far as I know, in no other country in the world, a number of examinations, of which the Mathematical Tripos at Cambridge, and Greats at Oxford, have been the outstanding examples, which are of quite another type, and which fulfil, or purport to fulfil, quite different and very much more ambitious ends. These examinations originate in Oxford and Cambridge, and are found in their full development there only, though they have been copied to a certain extent by our modern universities. They are described as "honours" examinations, and pride themselves particularly on their traditions and their "standards". To these examinations are subjected a heterogeneous mass of students of entirely disparate attainments, and the examination professes to sort out the candidates and to label them according to the grade of their abilities. Thus in the old mathematical Tripos there were three classes, each arranged in order of merit, while in the new there are three classes and two degrees of marks of special distinction. It is evident that such an examination is not content with fulfilling the ends which I have admitted that an examination can fulfil so well; it is not, and prides itself that it is not, merely a useful test of industry, intelligence, and comprehension. It purports to appraise, and it must be admitted that to some extent, though very imperfectly, it does appraise, higher gifts than these. A "b*" in the Tripos, or a first in Greats, is taken to be, and in a measure is, an indication of a man quite outside the common run. It is these examinations and these only, these examinations with reputations and standards and traditions, which seem to me mistaken in their principle and useless or damaging in their effect, and which I would destroy if I had the power. An examination can do little harm, so long as its standard is low.

I suppose that it would be generally agreed that Cambridge mathematics, during the last hundred years, has been dominated by the Mathematical Tripos in a way in which no first-rate subject in any other first-rate university has ever been dominated by an examination. It would be easy for me, were the fact disputed, to justify my assertion by a detailed account of the history of the Tripos, but this is unnecessary, since you can find an excellent account, written by a man who was very much more in sympathy with the Tripos than I am, in Mr. Rouse Ball's *History of Mathematics in Cambridge*. I must, however, call your attention to certain rather melancholy reflections which the

history of Cambridge mathematics suggests. You will understand that when I speak of mathematics I mean primarily pure mathematics, not that I think that anything which I say about pure mathematics is not to a great extent true of applied mathematics also, but merely because I do not want to criticise where my competence as a critic is doubtful.

Mathematics at Cambridge challenges criticism by the highest standards. England is a first-rate country, and there is no particular reason for supposing that the English have less natural talent for mathematics than any other race; and if there is any first-rate mathematics in England, it is in Cambridge that it may be expected to be found. We are therefore entitled to judge Cambridge mathematics by the standards that would be appropriate in Paris or Göttingen or Berlin. If we apply these standards, what are the results? I will state them, not perhaps exactly as they would have occurred to me spontaneously—though the verdict is one which, in its essentials, I find myself unable to dispute—but as they were stated to me by an outspoken foreign friend.

In the first place, about Newton there is no question; it is granted that he stands with Archimedes or with Gauss. Since Newton, England has produced no mathematician of the very highest rank. There have been English mathematicians, for example Cayley, who stood well in the front rank of the mathematicians of their time, but their number has been quite extraordinarily small; where France or Germany produces twenty or thirty, England produces two or three. There has been no country, of first-rate status and high intellectual tradition, whose standard has been so low; and no first-rate subject, except music, in which England has occupied so consistently humiliating a position. And what have been the peculiar characteristics of such English mathematics as there has been? Occasional flashes of insight, isolated achievements sufficient to show that the ability is really there, but, for the most part, amateurism, ignorance, incompetence, and triviality. It is indeed a rather cruel judgment, but it is one which any competent critic, surveying the evidence dispassionately, will find it uncommonly difficult to dispute.

I hope that you will understand that I do not necessarily endorse my friend's judgment in every particular. He was a mathematician whose competence nobody could question, and whom nobody could accuse of any prejudice against England, Englishmen, or English mathematicians; but he was also, of course, a man developing a thesis, and he may have exaggerated a little in the enthusiasm of the moment or from curiosity to see how I should reply. Let us assume that it is an exaggerated judgment, or one rhetorically expressed. It is, at any rate, not a *ridiculous* judgment, and it is serious enough that such a condemnation, from any competent critic, should not be ridiculous. It is inevitable that we should ask whether, if such a judgment can really embody any sort of approximation to the truth, some share of the responsibility must not be laid on the Mathematical Tripos and the grip which it has admittedly exerted on English mathematics.

I am anxious not to fall into exaggeration in my turn and use extravagant language about the damage which the Tripos may have done, and it would no doubt be an extravagance to suggest that the most ruthless of examinations could destroy a whole side of the intellectual life of a nation. On the other hand it is really rather difficult to exaggerate the hold which the Tripos has exercised on Cambridge mathematical life, and the most cursory survey of the history of Cambridge mathematics makes one thing quite clear; the reputation of the Tripos, and the reputation of Cambridge mathematics stand in correlation with one another, and the correlation is large and negative. As one has developed, so has the other declined. As, through the early and middle nineteenth century, the traditions of the Tripos strengthened, and its importance in the eyes of the public grew greater and greater, so did the external reputation of Cambridge as a centre of mathematical learning steadily

decay. When, in the years perhaps between 1880 and 1890, the Tripos stood, in difficulty, complexity, and notoriety, at the zenith of its reputation, English mathematics was somewhere near its lowest ebb. If, during the last forty years, there has been an obvious revival, the fortunes of the Tripos have experienced an equally obvious decline.

Perhaps you will excuse me if I interpolate here a few words concerning my own experience of the Tripos, which may be useful as a definite illustration of part of what I have said. I took the first part of the Tripos in 1898, and the second in 1900: you must remember that it was then the first part which produced wranglers and caught the public eye.

I am inclined to think that the Tripos had already passed its zenith in 1898. There had already been one unsuccessful attempt to abolish the order of merit, a reform not carried finally till 1910. When the first signs of decline might have been detected I cannot say, but the changes in the Smith's Prize examination, and the examination for Trinity Fellowships, must have been partly responsible, and these had been determined by dissertation for a considerable time. At any rate it was beginning to be recognised, by the younger dons in the larger colleges, and to some extent by undergraduates themselves, that the difference of a few places in the order of merit was without importance for a man's career. This, however, is comparatively unimportant, since it is less the examination itself than its effect on teaching in the university that I wish to speak of now.

The teaching at Cambridge when I was an undergraduate was, of course, quite good of its kind. There were certain definite problems which we were taught to solve; we could learn, for example, to calculate the potential of a nearly spherical gravitating body by the method of spherical harmonics, or to find the geodesics on a surface of revolution. I do not wish to suggest that the two years which I spent over the orthodox course of instruction—my second two years were occupied in a different way—were altogether wasted. It remains true that, when I look back on those two years of intensive study, when I consider what I knew well, what I knew slightly, and of what I had never heard, and when I compare my mathematical attainments then with those of a continental student of similar abilities and age, or even with those of a Cambridge undergraduate of to-day, it seems to me almost incredible that anyone not destitute of ability or enthusiasm should have found it possible to take so much trouble and to learn no more. For I was indeed ignorant of the rudiments of my profession. I can remember two things only that I had learnt. Mr. Herman of Trinity had taught me the elements of differential geometry, treated from the kinematical point of view; this was my most substantial acquisition, and I am grateful for it still. I had also picked up a few facts about analysis, towards the end of those two years, from Prof. Love. I owe, however, to Prof. Love something much more valuable than anything he taught me directly, for it was he who introduced me to Jordan's *Cours d'analyse*, the bible of my early years; and I shall never forget the astonishment with which I read that remarkable work, to which so many mathematicians of my generation owe their mathematical education, and learnt for the first time as I read it what mathematics really meant.

It has often been said that Tripos mathematics was a collection of elaborate futilities, and the accusation is broadly true. My own opinion is that this is the inevitable result, in a mathematical examination, of high standards and traditions. The examiner is not allowed to content himself with testing the competence and the knowledge of the candidates; his instructions are to provide a test of more than that, of initiative, imagination, and even of some sort of originality. And as there is only one test of originality in mathematics, namely the accomplishment of original work, and as it is useless to ask a youth of twenty-two to perform original research under examination

conditions, the examination necessarily degenerates into a kind of game, and instruction for it into initiation into a series of stunts and tricks. It was in any case certainly true, at the time of which I am speaking, that an undergraduate might study mathematics diligently throughout the whole of his career, and attain the very highest honours in the examination, without having acquired, and indeed without having encountered, any knowledge at all of any of the ideas which dominate modern mathematical thought. His ignorance of analysis would have been practically complete. About geometry I speak with less confidence, but I am sure that such knowledge as he possessed would have been exceedingly one-sided, and that there would have been whole fields of geometrical knowledge, and those perhaps the most fruitful and fascinating of all, of which he would have known absolutely nothing. A mathematical physicist, I may be told, would on the contrary have received an appropriate and an excellent education. It is possible; it would no doubt be very impertinent for me to deny it. Yet I do remember Mr. Bertrand Russell telling me that he studied electricity at Trinity for three years, and that at the end of them he had never heard of Maxwell's equations; and I have also been told by friends whom I believe to be competent that Maxwell's equations are really rather important in physics. And when I think of this I begin to wonder whether the teaching of applied mathematics was really quite so perfect as I have sometimes been led to suppose.

I remember asking another friend, who was Senior Wrangler some years later, and has since earned a very high reputation by research of the most up-to-date and highbrow kind, how the Tripos impressed him in his undergraduate days, and his reply was approximately as follows. He had learnt a little about modern mathematics while he was still at school, and he understood perfectly while he was an undergraduate, as I certainly did not, that the mathematics he was studying was not quite the real thing. But, he continued, he regarded himself as playing a game. It was not exactly the game he would have chosen, but it was the game which the regulations prescribed, and it seemed to him that, if you were going to play the game at all, you might as well accept the situation and play it with all your force. He believed—and remember, if you think him arrogant, that his judgment was entirely correct—that he could play that game at least as well as any of his rivals. He therefore decided deliberately to postpone his mathematical education, and to devote two years to the acquisition of a complete mastery of all the Tripos technique, resuming his studies later with the Senior Wranglership to his credit and, he hoped, without serious prejudice to his career. I can only add—lost as I am in hopeless admiration of a young man so firmly master of his fate—that every detail of these precocious calculations has been abundantly justified by the event.

I feel, however, that I am laying myself open at this point to a challenge which I shall certainly have to meet sooner or later, and which I may as well deal with now. It will be said—I know from sad experience that such things are always said—that I am applying entirely wrong criteria to what is after all an examination for undergraduates. I shall be told that I am assuming that the principal object of the Cambridge curriculum is to increase learning and to encourage original discovery, and that this is false; that learning and research are admirable things, but that a great university must not allow itself to be overshadowed by them; and that, in short, a German professor of mathematics, however universal his reputation and profound his erudition, is not necessarily the noblest work of God. Indeed, at this point I seem to hear the voice of my opponent grow a little louder, as he points out to me that I am entirely misconceiving the function of an English university, that the universities of England are not at all intended as machines for the generation of an infinite sequence of professors, but as schools for the development of intellect and character, as training grounds of teachers, civil servants, states-

men, captains of industry, and proconsuls, in short as nurseries where every young Englishman may learn to add his quatum to the fulfilment of the destinies of an imperial race. I wish very heartily, I confess, that I was not going to be told all this, but I know very well that it is coming, for have I not heard it all a hundred times already, and did we not hear it all in 1910, from all the Justices who had been wranglers in their day?

Perhaps, however, I shall not be wasting your time entirely if I occupy a few minutes in an attempt to examine this indictment as dispassionately as I can. I find it very difficult to believe that most of the quite considerable body of quite intelligent people who continue to use this kind of language at the present day, and to turn it to the defence of our present university education, can have considered at all coolly some of the implications of what they say. On the other hand I recognise that it is a good deal easier to laugh at these people than to refute them, and that, if I were to attempt a reasoned reply to their contention, considered as a general principle to guide us in the construction of an educational system, then I should have a long and tiresome argument before me.

Fortunately, this is unnecessary. We are not now discussing educational systems generally, but the merits of a particular examination. We have not to undertake a general defence of mathematics and the position which is at present allowed to it in education, or to repel the very formidable onslaught which might be directed against it by Philistinism pure and simple. You and I and the Justices are after all agreed in wanting to see some sort of education in higher mathematics, and differ only in the kind of mathematical education which we prefer. The question is merely whether it is possible to defend the Mathematical Tripos on these lines, and we can appeal here, I think, to the method of *reductio ad absurdum*.

I have already put forward one test of a mathematical education, namely that it should produce mathematicians, as "mathematician" is understood by the leading mathematicians of the world; and this test, whatever its defects may be, has one merit at any rate, namely that it is clear and sharp and easily applied. It is also a test to which I suppose that everybody would agree in attaching some degree of importance, since it must be extraordinarily difficult for any English mathematician to maintain that it is of no importance whatever whether English mathematics be good or bad. The question therefore is not of the validity of the test, but only of its relative importance.

Now there is one obvious difference between my test, which I will call for shortness the professional test, and the slightly more orotund test which I have tried to state in general terms. My test has certainly this advantage, that I am testing a mathematical education as a means to one of the ends which a mathematical education may reasonably be expected to secure, and which it is hardly possible to secure in any other way. When, on the other hand, we attempt to test a training in higher mathematics, the highest such training the country offers, by its effects generally on the intelligence and character of those who submit to it, we are at once confronted with a question which is obviously more fundamental, whether intelligence and character of the type at which we aim are really developed very effectively by a training in higher mathematica. And as we are all mathematicians here, we need not indulge in humbug about it. We know quite well that the answer is No.

It is hardly likely that anybody here will accuse me of any lack of devotion to the subject which has after all been the one great permanent happiness of my life. My devotion to mathematics is indeed of the most extravagant and fanatical kind; I believe in it, and love it, and should be utterly miserable without it, and I have never doubted that, for any one who takes real pleasure in it and has a genuine talent for it, it is the finest intellectual discipline in the world. I believe also that a fair knowledge of mathematics is, even for those who have no pronounced mathematical talent, extremely useful and

extremely stimulating, and that it should be part of the ordinary intellectual capital of all intelligent men. I am prepared indeed to go further, since I believe that a very large proportion of students abandon mathematics merely because it is often very badly taught, and might push their mathematical studies a good deal further than they do at present with very great profit to themselves. But I do not believe for a moment, and I do not believe that the majority of competent mathematicians believe, that the intensive study of higher mathematics, whether it be understood as it would be in a foreign university, or whether it be understood as it has in the past been understood in the Mathematical Tripos, forms a good basis of a general education. I am not at all sure that, among all possible subjects which might be selected as special courses of study, for an intelligent young man of no particular talent, mathematics is not the worst. Indeed, I think that this is being gradually recognised both by teachers of mathematics and by students themselves, and that it is for this reason that the Mathematical Tripos is more and more becoming, and rightly becoming, the special preserve of professional mathematicians. And if this be so, then surely it is quite obviously futile to judge the Tripos by anything but a professional standard.

It seems to me, then, that the opponents of the professional standard are committed from the beginning to a very paradoxical position, and yet it seems—such is the attraction of a paradox—that they are actually dissatisfied with its already sufficiently serious difficulties and determined to surround it by still more fantastic entrenchments. For they generally go on to maintain that mathematics may indeed be made the finest of intellectual disciplines, but only if it is taught in a manner which ignores or rejects every development of recent years. It will teach you to think, so long as you are not allowed to think quite correctly; it will widen your interests and stimulate your imagination, so long as you are carefully confined to problems in which mathematicians have lost interest for fifty years. In a word, the mathematics of the amateur is all right, and that precisely because it is so much more than a little wrong, but if we once allow mathematics to be dominated by the professionals, that is to say by the men who live in the subject and are familiar with its vital developments, then its energy will be sapped and its educational efficacy destroyed. And of all insane paradoxes, surely, this is one of the most portentous.

I have told you already that I am not much of a believer in the general educational efficacy of a specialised mathematical training. I do not believe that it is possible to build a character or an empire on a foundation of mathematical theories; but surely it must be still more impossible to build either on a foundation of Tripos problems. If I were compelled to undertake so crazy an enterprise, I would select the true theorems rather than the false, the fundamental facts of mathematics rather than its trivial excrescences, the problems which are alive to-day rather than those which perished in the mid-Victorian era.

I would suggest to you, then, that, when you have next to listen to the mathematical reactionary who laments the good old days, if you doubt your competence to judge for yourself the merits of his complaints, you should apply to what he says Hume's test of the greater improbability. It does not seem very likely that the modern experts are all wrong, but it is quite possible. It is also possible that the times have really left a conservatively-minded mathematician a little bit behind; that his lectures and his text-books have run out of date; that there is a good deal in modern mathematics which he finds it too great an effort to master; and that it gives him a good deal less trouble to abuse the modern tendencies than to repair the gaps in his own mathematical equipment. This also is, of course, extremely improbable; but you must ask yourself which is the greater improbability of the two.

I do not propose to waste further time on the discussion of this question : in what more I have to say about the Tripos I shall adopt a frankly professional view. I shall judge the Tripos by its real or apparent influence on English mathematics. I have already told you that in my judgment this influence has in the past been bad, that the Tripos has done negligible good and by no means negligible harm, and that, so far from being the great glory of Cambridge mathematics, it has gone a very long way towards strangling its development. There are further questions to consider. We may ask in the first place, if it be granted that what I have said about the past is roughly true, how far have things improved ? Is it not true already that the Tripos means a great deal less, and English mathematics appreciably more, than forty years ago, and is it not extremely likely that, even if there be no further radical changes, this process will continue ? Then, if we are not content to answer this question by a simple affirmative and leave it there, we may ask what really are the fundamental faults of an examination on the Tripos model, and whether it is not possible to make less drastic suggestions for its improvement.

I began my address with what was to a certain extent a defence of examinations. I said that under certain conditions I believed in examinations, that is to say in examinations of a sufficiently lowly type, which do not profess to be more than a reasonable test of certain rather humdrum qualities. The phrases which I used were vague, and I ought no doubt to attempt to define my own standard a little more precisely. This is naturally not quite easy, but I will risk some sort of definition. I should say, roughly, that the qualities which I have in mind—reasonable industry, reasonable intelligence, reasonable grasp—would be about sufficient to carry a candidate, in any of the orthodox Oxford or Cambridge examinations, into a decent second class. Beyond that, I do not believe in recognising differences of ability by examination.

I said this here last year, and I was at once challenged. I was asked, whatever could you do, if you could not tell the quality of a man by looking at his examination record ? I wonder whether my questioner realised that these elaborate honours examinations, so far from being one of the fundamental necessities of modern civilisation, are a phenomenon almost entirely individual to Oxford and Cambridge, copied in a half-hearted fashion by other English universities, and, beyond that, having hardly a parallel in the world ? Does Germany suffer from intellectual stagnation, because there are no honours examinations in her universities ? Germany does not think in terms of firsts and seconds ; we think in terms of them, so far as we do so think, and perhaps the practice is to some extent abating, merely because we have heard so much about them that they have become to us like bitter ale or eggs and bacon, and we have forgotten that we could get on quite happily without them.

I remember, if you will excuse my referring once more to the forgotten controversies of 1910, a curious saying of, I think, Mr. Justice Romer. Mr. Justice Romer circulated a flysheet to the Senate, deploring, of course, the proposal to abolish the Senior Wrangler. "What", he asked judicially, "is the function of the Tripos ?", and he replied "Surely to examine and to make distinctions between young men". It would indeed be difficult to compress a larger quantity of vicious educational doctrine into a smaller number of words. The exactly opposite doctrine, that no distinctions should be made by examination except such as practical necessities may make imperative, is surely somewhere a little nearer to the truth.

Let us then consider, with the view of meeting the objection which was raised to what I said last year, whether the kind of distinctions made by the Mathematical Tripos are, in fact, of any particular practical utility. The evidence of ability provided by the Tripos is as follows. A candidate may obtain a first, second, or a third. He may obtain a mark—the "b" mark—

of adequate knowledge of some special subject, or a higher mark—the “b*” mark—of special distinction in that subject. The test case for us, and the only one I have time to consider now, is the highest mark. When a candidate has attained this mark, what has he gained?

In the first place, he has gained the natural feeling of satisfaction which everyone experiences when he is adjudged to have performed a definite task at least as well as anybody else. He will feel with pleasure and pride that the world is awarding honest work, and these entirely creditable feelings may spur him on to further effort. Has he gained anything of more tangible or permanent value?

A man who can attain the highest honours in the Tripos is generally a good enough mathematician to hope for a permanent academic career. How far will his “b*” assist him along this career? Will anyone give him a position, a fellowship or a lectureship, on the strength of it? If he thinks that, he will be very quickly disillusioned.

It is possible that there are positions, in the junior grades of the teaching staffs of certain universities, which are sometimes filled on the strength of an examination record. I have never come across such a post myself, but it is probable enough that they exist. Academic positions are usually bestowed, not on examination record, but from personal knowledge or on the strength of private recommendations from competent people. I have taken part myself in many such appointments. When applications are invited, the testimonials submitted by candidates contain statements of their academic qualifications, and often of their performance in examinations, and it would be an exaggeration to say that such records are never referred to. There are usually a fair proportion of the candidates whose qualifications seem obviously below the standard expected, and a glance at their examination records often provides useful evidence in confirmation of this view. I do not remember any case of any other kind in which such a record has played any part in the decision, or has been referred to in the discussion by any member of the board of electors.

I suppose that this is generally understood, and that candidates for such positions are not usually under any delusion about the attention paid to the records which they submit. It may, however, be urged that an examination record of high distinction might often determine the decision if the post were of a less purely academic kind, for example if it were a mastership in one of the big public schools. It may be so, but I must confess that—at any rate in the particular case which we are considering—I am uncommonly sceptical about it. In the first place, people who obtain “b*”s have usually scientific ambitions, and the last thing they want is a mastership in the most historic of public schools. It is not possible now for the richest or the most aristocratic school to obtain a really distinguished mathematician, even if it wants one, which of course in general it does not. Finally, even if the demand existed and the supply were there, the headmasters of the great public schools do not, so far as my experience has shown me, select their assistants in this way, but proceed much more in the spirit of a board of electors, though naturally in a more capricious and autocratic way.

My conclusion, then, is that the highest certificate of merit offered by the Tripos might just as well be scrapped, for all the influence it exerts on the careers of those who obtain it. I suppose, in fact, that the universities, and most of the other bodies in whose hands educational patronage is vested, have come in practice to very much the same conclusion as my own, that examinations are an admirable test of competence and industry, but ineffective and erratic as a test of any higher gift. The Government stands alone, so far as I know, in attaching a definite money value to an examination class, and even the Government stops short of rewarding the only mark which could plausibly pretend to be a mark of real distinction.

If such distinctions are in effect futile, why should we waste our time and our energies in making them, even if we were certain that they do no harm? If Einstein had taken the Mathematical Tripos, what would it matter what place he took? The world can recognise its Einsteins quickly enough when it gets the opportunity. If Einstein sits for an examination, let him have his degree, assuming that he can satisfy the examiners. What is the object of taking all these pains to make to-day, uncertainly and half-heartedly, distinctions which, if they have any foundation in reality, the world will make in its own much sharper fashion to-morrow?

I have left to the last the defence of the Tripos which I find myself most difficult to meet. It is a defence difficult to overcome, because it proceeds on what a chess-player would call close lines. This defence, which I have often heard from mathematicians whose judgment I value, and which I wish to treat with all respect, is simply this: that the examination has already been considerably relaxed, and that the effects of its relaxation can already be traced in a corresponding strengthening of English mathematics; that it may be indefensible in principle, but that the spirit of emulation which it fosters may conceivably do some slight positive good; and that, now that so many of its teeth have been successfully drawn, it is not very obvious that it does any very serious harm. This is undeniably the case for the Tripos in its strongest and sanest form.

I should admit that, up to a point, the defence is sound. I would go so far as to admit that the system now does little harm to men of what I may define roughly as fellowship standard. The truth is that the principles for which I am contending have been so far recognised that a man of this degree of ability need not really disturb himself very seriously about the examination. Such a man may pursue a course of serious mathematical study with every confidence that, unless he is wilfully neglectful, he can obtain without any intensive effort all such honours as the Tripos can bestow. The test, in short, does him no harm, because for him it has lost its meaning. This I admit, and, of course, I recognise that it is a very large admission, since it destroys a good deal of the case which could be urged so irresistibly against the Tripos thirty years ago on strictly professional grounds. It is no longer true that Cambridge is notably behind the times, or that its courses compare particularly unfavourably, at any rate in the subjects about which I am best qualified to judge, with those at any but the very best of continental universities. This, I think, Mr. Heywood did not recognise sufficiently; it was the only point in his address from which I particularly dissented. It is no longer true that the development of a decent school of English mathematics is being steadily throttled by the vices of its principal examination.

We must recognise this and rejoice that things have moved so far, and if they have moved just because the glamour of the Tripos has faded, we shall only rejoice the more. We need not rush to the conclusion that the whole case against the Tripos has been destroyed. We have to think of its effects, not only on students of the highest class, but also upon teachers of mathematics in the university, and upon students a little less gifted than those of whom I have spoken hitherto. I am afraid that it is still true that mathematical teaching is hampered very seriously by the examinations, both in Cambridge and in Oxford, where the system is different in detail, but in essentials the same.

In the first place, it is still true that a large proportion of students, either wilfully, because they exaggerate the importance of the examination, or from ignorance, because they have never heard of anything better, or (and I am afraid that this is the most common explanation) because they are driven to it by tutors who have to justify themselves in the eyes of college authorities greedy for firsts, for one or other of these reasons allow their mathematical education to be stunted by absorption in examination technique. They

spend hour after hour, which ought to be devoted to lectures or reading, in working through examination papers, or the collections of problems in which English text-books are so rich, exhausting themselves and their tutors in the struggle to turn a comfortable second into a marginal first. It is possible that the effect of all this mistaken exertion is more directly damaging to the tutors than to the students themselves; but a pupil cannot draw much inspiration from a tutor who is always tired, and there is hardly a tutor in Oxford, and not very many in Cambridge, who has not about twice as much teaching as any active mathematician should be asked to undertake. A professor at Oxford or Cambridge is very much his own master, but even a professor may be handicapped very seriously in his teaching by the recollection of the syllabus of the schools. It is often very hard to ask your pupils to go on listening to you when you know that what you tell them will gain them no credit in an examination to which they attach enormous importance and over which you have practically no control. I should always like to ignore the examination completely, and often summon up the courage to do so for a while, only to be pulled up short a few weeks later by the thought that after all it is hardly fair.

Whatever, then, may be said about the improvement of Cambridge teaching, and however much the dominion of the Tripos may have abated of its rigours of thirty years ago, I adhere to the view which I expressed to you last year, that the system is vicious in principle, and that the vice is too radical for what is usually called reform. I do not want to reform the Tripos, but to destroy it. And if you ask me whether the Tripos is a peculiar case, or whether what I have said applies to all other high-grade honours examinations, I can only answer that, so far as I can see, it does. The Tripos is the worst case. It is the oldest examination and the most famous, and generally the most strongly entrenched; and mathematics is a subject in which it is particularly easy to examine ferociously, so that the evils of the system stand out here in the clearest light. But, of course, the greater part of what I have said about the Tripos could be applied with almost equal force to Greats.

I wish, then, to abolish the Tripos, and as I know perfectly well that neither I nor anyone else will succeed in doing so, since the practical difficulties would be so serious, and the force of tradition is so terribly strong, I may reasonably be asked what seem to me the best practical steps for a more moderate reformer to take. You will probably have inferred from my remarks that I am not prepared with any very illuminating suggestions. I could, of course, suggest many changes of detail, both in the schedules and in the conduct of the examination; but my suggestions would be comparatively unimportant; and I should not be prepared to expend my energies in pressing them, since they would all be inspired by the same ideal, and that an ideal whose realisation will no doubt remain hopeless for many years. Indeed I am afraid that my advice to reformers might sound like a series of stupid jokes. I should advise them to let down the standard at every opportunity; to give first classes to almost every candidate who applied; to crowd the syllabus with advanced subjects, until it was humanly impossible to show reasonable knowledge of them under the conditions of the examination. In this way, in the course of years, they might succeed in corrupting the value of the prizes which they have to offer, and in all probability time would do the rest.

348. When David Ramsay was asked to consent to his daughter's wedding, "He multiplied six figures progressively, and reported the product—then gave his consent."—*The Fortunes of Nigel*, c. xxxv.

349. Dodd was one of the rare examples of an eloquent mathematician.—Sir Charles Biron. *Johnson Club Papers*, 1920. ["Dodd, William, 1720-1777, forger" (*D.N.B.*). 15th Wrangler, 1749.]

MODERN THEORIES OF INTEGRATION.

By E. C. FRANCIS, M.A., FELLOW AND LECTURER OF PETERHOUSE,
CAMBRIDGE.

As the outcome of a short but occasionally painful experience of mathematical papers, I would suggest that such a paper, if it is to be of any interest at all to the majority of those to whom it is addressed, must necessarily be boring to some of them. It is therefore with this explanation that I am trying to put before you results which are not in any sense new, which are, indeed, the stock-in-trade of the present-day analyst, and therefore well known to many here, but which may interest some whose studies have not lain in this particular direction.

Implicit in the conception of an integral are *two* ideas—the inverse of a differential coefficient and the limit of a sum. These ideas are quite distinct. They afford us two avenues of approach. Either may be used to define an integral, the other being proved as a property of the integral so defined; and if our purpose is simply to act as calculating machines for the engineer and the physicist it does not matter very much which procedure we adopt. But if we are mathematicians, if we believe that mathematical theorems and processes are of some value in themselves, quite apart from bridges and waves and atoms, then it *does* matter, and we seek that theory which will be of greatest elegance, of greatest comprehensiveness, and capable most readily of extension. These considerations all lead us in one direction.

The majority of school text-books, for reasons into which I need not enter but which are probably sound, define an indefinite integral as the inverse of a differential coefficient, i.e. as a "primitive." Thus

$$F(x) = \int f(x) dx \quad \text{if} \quad F'(x) = f(x).$$

The schoolboy is confronted at once with what appear to be a series of anomalies.

He sees three functions, $\frac{1}{x^2}$, $\frac{1}{1+x^2}$, $\frac{1}{1-x^2}$, which appear to be very similar.

He is told that their integrals are respectively

$$-\frac{1}{x}, \quad \tan^{-1}x, \quad \frac{1}{2} \log \left(\frac{1+x}{1-x} \right).$$

He encounters e^x and e^{-x} , and tries to write down their primitives. He finds that this is the simplest thing in the world in the case of the first, and quite impossible in that of the second.

In differentiation he had something definite to do, to form

$$\frac{f(x+h) - f(x)}{h}$$

and to examine its limits. In his integration he has no corresponding direct process. If he cannot hit upon the integral, he has little to do but hope.

The result may not be disastrous to his mathematical career, but he is certainly to be pardoned if it confirms him in views which he has long held in his innermost being as to the absurdity of the whole subject.

There is much to be said for the schoolboy's point of view. It has its weaknesses—in particular it confuses the *existence* of a primitive with the existence of one *which can be written down in terms of a few elementary functions*—but it has singled out two grave objections to the "primitive" definition. Such a definition leads to an essentially tentative and haphazard process; it gives no immediate answer to the general questions:

(i) Does $\int f(x) dx$ exist?

(ii) If we know that it exists, how can we find its value?

Integration becomes a game of hunt the thimble with no one to say whether you are hot or cold, and with the considerable added complication that there may be no thimble; a game which, I suppose, typifies the mathematician's anathema.

And not only this. It fails to integrate very simple functions. Suppose that

$$f(x) \begin{cases} = 0 & \text{when } x < 0, \\ = 1 & \text{when } x \geq 0. \end{cases}$$

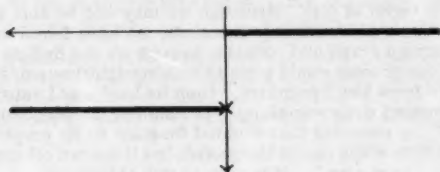


FIG. 1.

Suppose that a primitive $F(x)$ exists so that $F'(x) = f(x)$. We have

$$\frac{d}{dx}\{F(x)\} = 0 \quad \text{when } x < 0;$$

$$\therefore F(x) = A \quad \text{when } x < 0.$$

$$\frac{d}{dx}\{F(x) - x\} = 0 \quad \text{when } x \geq 0;$$

$$\therefore F(x) = x + B \quad \text{when } x \geq 0.$$

The only hope of $F(x)$ having a differential coefficient at $x=0$ is that it should be continuous, *i.e.* that $A=B$. But the function

$$F(x) \begin{cases} = A & \text{when } x < 0, \\ = A + x & \text{when } x \geq 0, \end{cases}$$

has no differential coefficient at all when $x=0$. Thus, on this definition, $f(x)$ has no integral.

But you protest. You say that $F(x)$ goes wrong at one point only; surely we may allow this minor lapse. But if one, then why not two? If two ..., and by an argument familiar in other than mathematical circles we do not know where to draw the line. Only by really complicated considerations can $F(x)$ be rescued.

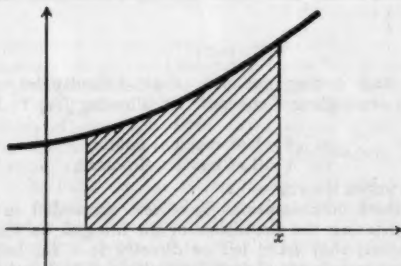


FIG. 3.

If one were concerned for the defence of the "primitive" definition, I suppose that one would naturally introduce the conception of the "area under the curve." If $A(x)$ represents the area up to the ordinate at the point x ,

then we are all familiar with the argument which shows that, under certain fairly general conditions, when x is changed to $x+h$, $A(x)$ is increased by about $h \cdot f(x)$, i.e.

$$\frac{A(x+h) - A(x)}{h} = \text{about } f(x) \rightarrow f(x) \text{ as } h \rightarrow 0;$$

$$\therefore A'(x) = f(x),$$

i.e. $A(x)$ is an integral of $f(x)$. Although we may not be able to write down an integral in terms of elementary functions, we have found one; by some method of drawing a graph and counting squares we can find its value as near as we please; our process would seem to be a straightforward, if a cumbrous, one and to have freed the "primitive" from its haphazard nature.

Such an argument does something. It removes the objection on score of injustice to e^x , by restoring that slighted function to its proper place alongside e^x as a function which can be integrated, but it has not otherwise improved the case for the "primitive." It is open to two objections.

In the first place the proof that $A'(x) = f(x)$ is true only "under conditions," and such conditions, though they may be general, are far from universally true. In the special case recently considered we proved that *no process whatever* can produce a primitive. Actually the area process produces exactly the function $F(x)$ which we found, and therefore $A'(x)$ fails at one point to equal $f(x)$.

Secondly—a more fundamental objection still—what is an area? We know what we mean by the area of a triangle and so of a polygon, but the area of a curve presents far greater difficulties. The definition must be an analytical one, for analysis must never rest on geometrical intuition. Will the area be the same when we rotate the axes? Do we know what we are

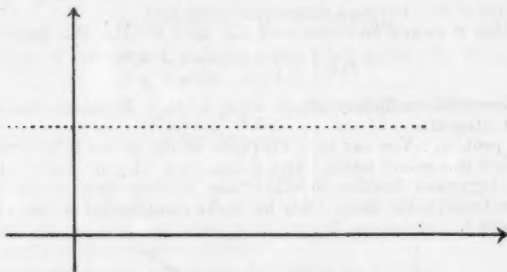


FIG. 4.

talking about? And having got over that difficulty let us consider the sort of things we are calling "curves" by allowing $f(x)$ to be rather more skittish. If

$$f(x) = \phi(x) \begin{cases} = 1 & \text{at rational points,} \\ = 0 & \text{at irrational points,} \end{cases}$$

what is the "area under the curve"?

But although these considerations have not succeeded in their original purpose of rehabilitating the definition of an integral as the inverse of a differential coefficient, they have led us directly to a far better definition. The area under the curve is not always a primitive of $f(x)$, but it is a function connected with $f(x)$ to which a definite meaning can be assigned, and it has important properties. Why not abandon the original search for a primitive—a search which we have shown to be unsatisfactory—and seek instead to attach an analytical meaning to an "area under the curve"? This is precisely what is done by the modern theories of integration.

The first and the simplest of the modern integrals bears the name of Riemann, though the definition which I propose to give is not his, but is due to Darboux. It makes one great assumption, that $f(x)$ is *bounded*, i.e. that m and M exist such that $m \leq f(x) \leq M$ for all x in the interval (a, b) in question.

The least value of M and the greatest value of m we define respectively as the *upper and lower bounds* of $f(x)$ in (a, b) .

Divide (a, b) by points $x_0 (=a), x_1, x_2, \dots, x_n (=b)$, in this order. Let M_r, m_r be the bounds of $f(x)$ in the r th interval (x_{r-1}, x_r) . Consider the two "partial sums"

$$S = \sum M_r (x_r - x_{r-1}), \quad s = \sum m_r (x_r - x_{r-1});$$

both definite quantities. It is clear that $S \geq s$, and it can easily be shown that every $S \geq$ every s . It follows that if I is the lower bound of the S 's and I' the upper bound of the s 's, then $I \geq I'$. If $I = I'$ we say that $f(x)$ is integrable (R) (i.e. integrable in the Riemann sense) from a to b , and we define

$$\int_a^b f(x) dx = I = I'.$$

Compare the integral so defined with the "primitive" mentioned before. We have now a *definite process* whereby, with *any* given bounded function $f(x)$, we can decide whether an integral exists or not, and can find its value. There is nothing haphazard about it.

The process will not integrate *every* function—for example it will not integrate the function $\phi(x)$ mentioned before, for we have always $S = b - a$ and $s = 0$, and therefore $I = b - a$ and $I' = 0$; but it will integrate most functions—every continuous function and every increasing or decreasing function. And for *every* function (provided only that it is bounded, as originally assumed) we have got I and I' , which we call the upper and lower integrals,

$$\int_a^b f(x) dx \quad \text{and} \quad \int_a^b f(x) dx.$$

One naturally investigates the differential properties of the Riemann integral, and a necessary preliminary is to define an indefinite, instead of a definite, integral. This is naturally given by

$$F(x) = \int_a^x f(t) dt + C,$$

where C is an arbitrary constant. It is not difficult to prove that *at any point at which $f(x)$ is continuous $F'(x)$ exists and is equal to $f(x)$* . Conversely, if $F'(x)$ is continuous,

$$\int_a^b F'(x) dx = F(b) - F(a).$$

But these are *properties* of our integral, not its definition. The connection between the Riemann integral and the primitive of $f(x)$ is thus established.

In a recent presidential address to the London Mathematical Society, Professor W. H. Young stated that "the acknowledgment and singling out for study of the theory of sets formed the characteristic great and notable progress of mathematics in this first quarter of a century". Everywhere it has extended its domain of usefulness, and now it forms the basis of modern analysis. Under its influence the Riemann integral just defined has lost its primary position: when an integral occurs in an analytical work to-day it is normally supposed to be a Lebesgue integral.

The Riemann integral depended essentially upon the division of the range of integration (a, b) into a finite number of intervals I_r , and the consideration of the partial sums

$$S = \sum M_r \delta_r, \quad s = \sum m_r \delta_r,$$

where δ_r is the length of I_r . But why intervals? One naturally enquires, what would be the result of dividing (a, b) into *sets of points* E_r , and considering similar partial sums. M_r, m_r will now represent the bounds of $f(x)$ on E_r ,

while δ_r will be some number which bears the same relation to E_r as did the length δ_r to the interval I_r . This quantity we denote by mE_r and call the *measure* of E_r . It was the introduction of this concept which first gave to sets of points their pre-eminent position.

The measure of a set E may be defined in various ways. In the case when E is an interval, mE must be the length of that interval. In other cases we consider a set of a finite or infinite number of intervals a which contain E . The lower bound of $\sum ma$ for all such sets of intervals a we define as the *upper measure* or *outer measure* of E and denote by m^*E . If E is contained in an interval I and if CE is its complementary set (i.e. the set of points of I which do not belong to E), then we define the *lower* or *inner measure* of E by

$$m_*E = mI - m^*CE.$$

If $m^*E = m_*E$ we say that E is "measurable," and define

$$mE = m^*E = m_*E.$$

No non-measurable set has ever been produced: we cannot say for certain whether such exist or not. Probably we shall never know.

It may be of interest to find the measure of some set, and I choose the rational points in $(0, 1)$. There are at most n of them having denominator n . Surround each such point by an interval of length ϵ/n^2 . The total length of these intervals is less than ϵ/n^2 ; and the total lengths of the intervals for all n is therefore less than $\epsilon \sum n^{-2}$, i.e. ϵA , where A is an absolute constant. We have thus enclosed the rational points of $(0, 1)$ in a set of intervals whose total length is as small as we please. It follows that $m^*E = 0$.

But since $m_*E \geq 0$ and $\leq m^*E$ it follows that

$$m_*E = m^*E = 0,$$

i.e. E is measurable and has measure 0.

The Lebesgue integral is thus defined (again my definition is not that originally given, but a later definition, due to W. H. Young) by following exactly the same lines as those used for the Riemann integral, only using a division of (ab) into measurable sets of points E_r and the sums $\sum M_r mE_r$ and $\sum m_r mE_r$ in place of the division into intervals and the sums $\sum M_r \delta_r$ and $\sum m_r \delta_r$. Clearly the Lebesgue integral is more comprehensive than the Riemann, for the division into intervals is only a particular case of that into sets. Wherever the Riemann integral exists, so does the Lebesgue, and they are equal.

The Lebesgue process integrates every known bounded function—if there are no non-measurable sets it integrates every bounded function. We can define an indefinite integral $F(x)$ as before, and prove the interesting differential property that $F'(x) = f(x)$ *almost everywhere* (i.e. except at points of a set of measure zero).

The Lebesgue integral of $\phi(x)$ (the function previously defined) is easily seen to exist. For divide the interval $(0, 1)$ into the sets of rational and irrational points, E_1 and E_2 respectively. We have $M_1 = m_1 = 1$, $M_2 = m_2 = 0$, $mE_1 = 0$, $mE_2 = 1$. It follows that $S = s = 0$, so that $I = I' = 0$.

At this stage I might point out the possibility of extension to more than one dimension. By dividing a rectangular area into rectangles R_r of areas a_r and considering $\sum M_r a_r$, $\sum m_r a_r$, we obtain a double Riemann integral: by dividing into sets of points we obtain a double Lebesgue integral. Three and more dimensions present no difficulty whatever. But what was the extension of the "primitive" to such?

Can we go further? One great restriction remains— $f(x)$ has been assumed bounded throughout. Though beaten at almost every point by these fabrications of the analyst, the primitive can still hold up its head here. Even when $f(x)$ is not bounded it may have a primitive: it cannot have a Riemann or a Lebesgue integral. Can we not define an integral on Lebesgue lines to beat the primitive here?

de la Vallée Poussin defined a makeshift extension of the Lebesgue integral to deal with certain unbounded functions, but did not succeed in dealing with all cases which are dealt with by a primitive. The integral produced is absolutely convergent, i.e. $\int_a^b f(x) dx$ exists only when $\int_a^b |f(x)| dx$ does so. It remained for Denjoy to complete the theory by producing an integral which he showed to exist and to be discoverable by a determinate process whenever a primitive existed—indeed whenever a continuous function $F(x)$ existed which had $f(x)$ for one of its generalised derivatives at all points.

The Denjoy integral is produced from the Lebesgue by a complicated series of operations, the set of "awkward points" being gradually reduced as the stages advance. Its importance lies in the fact that, in the cases mentioned, the process may be shown to terminate satisfactorily. It is not necessarily absolutely convergent. It is remarkable that to this amazingly general integral the usual theorems on Integration by Parts, Change of Variable, and Second Mean Value, can be shown to extend.

In conclusion I might make mention of another direction in which extension has proved possible and fertile. The Denjoy integral marks the furthest advance in one direction, but an entirely new march is opened if a function $\phi(x)$ is substituted for the variable x of integration. In connection with the *Stieltjes* integral

$$\int_a^b f(x) d\phi(x),$$

we can consider sums such as $\sum M_r(\phi(x_r) - \phi(x_{r-1}))$ instead of $\sum M_r(x_r - x_{r-1})$, and so define a *Riemann-Stieltjes* integral. $\phi(x)$ must for this purpose be of bounded variation. We can extend to a *Lebesgue-Stieltjes* integral. By allowing $\phi(x)$ to be of non-bounded variation, or $f(x)$ to be violently unbounded, we obtain a non-absolutely convergent integral which may be called a *Denjoy-Stieltjes* integral, and which includes as special cases all the other types of integral previously mentioned.

The PRESIDENT: I should like to add a few words, in thanking Mr. Carey Francis for his paper. I hope the members of the Association realise that in order to be a serious mathematician it is necessary to have some knowledge of modern theories of integration. To be a serious pure mathematician and not to use the Lebesgue integral is to adopt the attitude of the old man in a country village who refuses to travel in a train. It is necessary to learn these things and to get rid of the sort of terror which they appear to engender. It is true that the Lebesgue integral is very much easier than the Riemann, though naturally the beginnings of it are bound to be a little more difficult. And it is true, in a sense, that the Riemann integral is riddled with awkwardness and exceptions, but when one gets beyond the root of the subject, then the integral of Lebesgue is not really that of generalisation, but of simplification.

There is only one point in Mr. Carey Francis' remarks with which I am disposed to quarrel. At the outset he appeared to me to pander rather too much to the popular view of the ordinary process of what is called indefinite integration and finding of rules on which one can calculate rather more than is strictly justified. When they are told, "There is a certain combination of functions, will you write down the function which is the root of those functions?" as a matter of fact mathematicians know how to solve that particular problem as well as any other. If the integral of a given function is a combination of elementary functions, then the mathematician will find that combination of elementary functions, if it is worth his while. What usually happens is that somebody brings the mathematician a function of which the integral is not elementary, and complains because the latter gives an elementary function. The answer to that is not that we should blame the mathematician, but the University.

DISCUSSION ON "THE PROPER FUNCTION OF THE MATHEMATICAL GAZETTE."

Mr. E. R. BROWN, B.A., in opening the discussion, said :

I wish to speak with the utmost brevity on the question of the proper function of the *Gazette*, for we have not much time, and it is desirable that as many members as possible should say whether they are satisfied with the *Gazette*, and if not, why not. Personally I find the *Gazette* rather dull ; I have asked a few mathematicians what they thought of it, and their replies were too abusive to be quoted. It is not the fault of the editor : he can only make use of what he is sent. It is the fault of those of us who never send anything.

I feel that the *Gazette* is too concerned with difficult elementary mathematics (I refer to such things as Morley's Theorem), and too much neglects easy advanced mathematics (as, for example, tetracyclic coordinates). The mathematical notes are frequently solutions of problems which we could mostly solve ourselves if we wished to, although why anyone should ever want (say) the directrices of the general conic in trilinear coordinates I cannot imagine, and if such an unfortunate need did arise for anybody he probably would fail to find the number in which it appeared.

I suggest that the notes do not interest the junior mathematical master because they are too difficult, nor the senior master because they are too elementary.

The articles frequently suffer from the same fault—do we really want articles on identities in elementary trigonometry, on conjugate diameters in areals, on the elements of vector analysis, on homogeneous products, etc., etc. ?

The methods which we work out for ourselves, although possibly intrinsically less meritorious, are pedagogically more valuable, since we can feel greater enthusiasm for them.

The more general articles are more interesting. There was an article two years ago suggesting research into the existence of a specifically mathematical ability and the possibility of its development which should have led to an important discussion, but unfortunately it fell on barren soil. I do not wish such articles to be excluded, nor would I be glad to lose such an article as that in the December number, in which is developed a radically new method of introducing the exponential function.

The reviews, written as we are told on the back of the *Gazette* by men of eminence, are usually good though somewhat tardy in appearance ; and the extraordinarily bad text-books which are constantly appearing or reappearing are occasionally dealt with too gently.

Having indicated what of the *Gazette* I should like retained and what given up, I shall now go on and indicate the sort of thing I wish to read, which at present has no place therein. Briefly this is—more mathematics. My interest in the subject did not evaporate as soon as I left Cambridge : to be quite accurate, it began then. I want to know what is being done by the men of eminence apart from the reviews they write. I want to know what is the present centre of interest in mathematical research. There is in *Science Progress* a review of work done, I understand about $\frac{1}{10}$ of it. Am I exceptionally dull, or is that the case with all of us ? I cannot afford to buy the *Proceedings of the London Mathematical Association*, for I should understand about the same proportion. Could not the *Gazette* act as an intermediary ; take work of importance from as many as possible of the mathematical journals, and write it down to our level ? For example, I should like to know of the extension of Heaviside's Operational Method of Solving Differential

Equations, which appeared in the *Proceedings of the Edinburgh Mathematical Society*, April, 1924. I should be interested in learning what Zindler had to say in the 33rd volume of the *Monatshefte für Mathematik und Physik* on multi-dimensional linear and planar complexes. Living in the wilds of Lancashire—and most of us live in some wilds or other—there is no opportunity of having such desires gratified without incurring an expenditure which the Burnham Scale does not provide for. Could not the *Gazette* do something to help us to know more of the subject, enthusiasm for which we are trying to inculcate into others?

We are not a wealthy Association and cannot purchase all mathematical journals, but I suggest that we could buy half-a-dozen or so of the more important ones. One number of the *Gazette* might be devoted to the geometrical papers, another to the dynamical papers, and so on. If in writing these more knowledge had to be assumed in the reader than is required for Schedule A of the Mathematical Tripos, references should be given so that the gap might be filled, if desired, and in no case should knowledge be assumed that is not obtainable in book form. Certain papers might be dealt with in the briefest possible manner, being confined, say, to a statement of the premises and the final results; others might be dealt with more fully, perhaps even to the extent of suggesting extensions: this would be a matter for the individual contributor to decide. It seems to me that this would be an extremely valuable work, and work, too, that is very suitable for capable mathematicians who are not men of eminence. The Association must contain many who would be glad of the opportunity of serving mathematics in this comparatively humble way.

I hope that as many members as possible will now express their opinions, so that the Council may obtain a clear idea of the feeling of the Association as represented here, and may take action upon it.

Mr. W. HOPE-JONES (Eton) spoke in defence of an article on Morley's theorem and the geometrical proof which appeared in the *Gazette* sometime ago, which certainly he would never otherwise have had an opportunity of seeing. He would be sorry, too, if his fellow-members were to be deprived of similar opportunities.

Mr. INMAN (Isleworth) pointed out that most of the members of the Association were teachers and had to do very largely with elementary work. He was sure they would like a good deal more space in the *Gazette* devoted to methods of dealing with elementary work. Most of the matter in the *Gazette* dealt with Sixth Form Work, and hardly ever went below that. He suggested also that there might be more frequent reports of papers read by and discussions among members of kindred associations published in the *Mathematical Gazette*. Such matter would be interesting to the members of the other associations as well as to the general body of members of the Mathematical Association.

Mr. KATZ (Croydon) contended that the *Gazette* served two definite purposes: on the one hand, it helped teachers to teach; on the other, and rightly, it ought to help them to continue their studies. The practical difficulty as he saw it was as to who was going to undertake the job of working through the papers, boiling them down, and sending in reports to the Editor. If Mr. Brown could outline any method by which that work could be done and the results published in the *Gazette*, then its value would be very much increased. Obviously they all wanted the *Gazette* to help the teacher in the difficult art of mathematical teaching; on the other hand, it was important that the publication should be kept up-to-date as to any specialised mathematical work.

Mr. E. R. BROWN thought there must be among the members of the Association at least half-a-dozen capable of dealing with certain branches of work. He knew two, one of whom was not a member of the Association but one who would be willing to give such time as he could to the work. He might be over

optimistic, but there were, he thought, half-a-dozen or a dozen members who were capable of and willing to undertake the necessary work.

Dr. F. S. MACAULAY thought it perfectly obvious that there could not be published in the *Gazette* anything like a *résumé* of all the mathematical literature that appeared elsewhere, because there were not sufficient funds available. In that case those who had taken the trouble to send in *résumés* of special work which they thought would be of interest would be discouraged to go on with such work. It would, moreover, be very difficult to decide what should be published and what should not. Probably most of the matter sent in would have to be rejected partly on account of lack of space, partly because some would consider it too advanced—and he did not think that even the Editor would like to take the responsibility of deciding what should go in under those circumstances and what should be left out.

Mr. E. R. BROWN: Supposing *A* was asked to deal with geometry, then the Editor would allot him a certain amount of space, and it would be up to *A* to deal with the material in what he judged the best possible way to fill that given amount of space. I do not see that there would be any difficulty about that.

A MEMBER (Slough) supported what had been said by Mr. Katz as to the purpose of the *Gazette*, but the danger lay in the fact that there had been an attempt to cater for both the teacher and the student by taking a middle path. It was possible to give something in between what would be useful to members as students and what was useful to them as teachers and useful to them as neither, and that had actually been done in the *Gazette*; they had been given something which was altogether beyond what, for the most part, was of any use in teaching classes, which at the same time was perhaps a little too elementary as helping teachers. He thought it was necessary to avoid that *via media*, so useful in certain other circumstances.

The PRESIDENT agreed that the problem was a very difficult one. It might interest members to know that it was dealt with by the American Mathematical Association, which was a somewhat bigger and richer association than their own. He did not know whether the members of the Association ever saw the two American papers which filled a position somewhere in between that of the *Mathematical Gazette* and the *Proceedings of the Mathematical Society*. There was an American *Mathematical Monthly*, a paper on the same general lines as the *Gazette*; in some ways better; in others not so good. On the whole, he thought the special articles were less interesting and the reviews also. On the other hand, the lowest level of the American *Mathematical Monthly* was not quite so low as the lowest level of the *Mathematical Gazette*; certainly the former was very much stronger on the historical side, but that was because the editor of the *Mathematical Monthly* was primarily a professional mathematician who was particularly interested in the historical aspect. The *Annals of Mathematics*, on the other hand, was definitely a paper which rather fulfilled the functions which Mr. Brown would like the *Gazette* to assume. That paper was not entirely published by the American Mathematical Association, but under an agreement with the American Mathematical Society, and represented a compromise between the Mathematical Association, which, to a certain extent, subsidised original mathematical research and enabled the paper to be kept going on the understanding that a certain amount of the space should be devoted to matter of mathematical pedagogy. He advised members to compare those two papers and see how far something on similar lines would meet the case.

Dr. F. S. MACAULAY asked if it was not the case that one—if not both—of those journals was devoted chiefly to original work. He was sure the *Gazette* would accept every piece of original work sent in, so long as it was not really too advanced. It seemed to him that the *Gazette* might almost be divided into two parts, one for such as had been suggested and the other to carry on the work of the *Gazette* as now.

The PRESIDENT thought the *Mathematical Monthly* contained nothing in the nature of original work, though it occasionally contained semi-popular articles giving accounts of recent research. Two-thirds of the *Annals of Mathematics* consisted of what was probably original work of the highest order of difficulty. He supposed at present the Editor accepted pretty much what was sent in.

Mrs. JESSIE WHITE thought a separation of the *Gazette* into two parts unnecessary. Everyone would know in which part to look for what interested him. It might be of interest to members to know what had been done in connection with the *Teachers' Quarterly* when there was an attempt to bring before its readers a general *résumé* of what other workers were doing. One set of magazines was allotted to one person, another set to someone else, and so on, and the whole thing worked quite easily.

Prof. H. T. H. PIAGGIO (University College, Nottingham) thought there was a great difference between giving an account on mathematical research and on other subjects. Supposing, for instance, somebody very good in elementary geometry endeavoured to give an account of recent progress in geometry, he would find it difficult to make anything out of modern geometry; in fact, specialisation in mathematics seemed to be carried to such an extent that it was doubtful whether an account of really modern work would be intelligible to anyone who had not taken a long course of preparation. While there might be semi-biographical articles on modern mathematicians, in which reference could be made to their recent work, an account of pure mathematical research would probably be unintelligible to all except those who had followed preceding papers.

The PRESIDENT took it that Mr. Brown would like more articles of the nature of the paper read by Mr. Carey Francis, which would appear in the *Gazette*. Personally, he did not think that mathematical subjects should be difficult of comprehension, at any rate if readers were prepared to take any trouble.

Mr. E. R. BROWN, in replying, said the President was right when he assumed that there should be more papers published in the *Gazette* of the type of that which Mr. Francis had read. It had introduced analysis, and was presented in such a manner that the majority of those present could understand it. He really could not see why that should not be done in connection with other subjects, even in the case of geometry. His object was that mathematics should be encouraged. He cared for mathematics, not merely for teaching the subject. He supposed the majority of people would take mathematics at the University, and many would teach it in the schools, but why end there? And that was where he felt the *Gazette* might be of greater assistance, in that it could give an indication of what there was to be done; an indication of the problems that needed solving and the way in which they might be solved.

The PRESIDENT, in closing the discussion, thanked Mr. Brown for opening what had proved to be a very instructive and vivacious debate.

350. Went out. After a brilliant college career he went out as Senior Wrangler.

351. Utopia. A satire, in imitation of a Mathematical Examination Paper: said to be written by a gentleman of Sidney Sussex College, A.D. 1816.—(Differs somewhat from that in *Gazette*, ix. p. 169.) *Facetiae Cantab.* pp. 46-47, 1836.

352. (Daniel Fournier, engraver and draughtsman), at about the age of fifty, wrote his book of perspective: during the time he was writing it, he used to draw the diagrams on the alehouse walls with chalk or porter, and was known by the appellation of the Mad Geometer.—F. Grose, *The Olio*, 1796, p. 165.

DISCUSSION ON "THE REPORT ON THE TEACHING OF MATHEMATICS TO EVENING TECHNICAL STUDENTS."

Prof. H. T. H. PIAGGIO, in opening the discussion, said: Mr. Chairman, Ladies and Gentlemen,—The subject of mathematics for technical students is a very interesting one, which the Mathematical Association has rather neglected. Evening technical students need mathematics much more than the ordinary school-boy, but their opportunities for acquiring it are much less. It is unfortunate that, owing to strikes in the printing trade and New Year celebrations in Scotland, we have not yet received the report prepared by the Sub-Committee, but a duplicated statement has been given out which will enable you to form an opinion on that report, and to express your views during the course of the discussion. The most important passages are the recommendations concerning practice in algebra and deductive geometry. The second subject is a somewhat controversial one, and I hope we may have a lively discussion upon it.

A point of a less general nature, but one that appeals probably to everyone present, whether engaged in Technical School or University work, is the question of how to introduce the exponential function and Napierian logarithms in a manner which is at once simple, natural and convincing. I do not think there is any method that combines all those conditions, but in Appendix III. are given three methods that answer some of them. Perhaps the most original is that due to Professor Nunn.

Mr. Holmes, Mr. Buxton and Professor Nunn will now deal in more detail with the three points mentioned, after which it is hoped that members will give us their views, either on the general recommendations or on the particular methods. Probably someone will have alternative methods to bring to our notice. I may add that as a result of the circulation of the draft report among technical teachers, writers of text-books and such bodies as the Institute of Mechanical Engineers, the Sub-Committee has received many criticisms, and the report has been amended to meet most, though not all, of those criticisms.

Mr. H. T. HOLMES (Board of Education, Technological Branch): Mr. Chairman, Ladies and Gentlemen,—Perhaps I might preface my remarks by saying that I am glad the Mathematical Association is taking an interest in this subject of teaching mathematics in Technical Schools. I think it merits that attention merely on account of its volume alone. It is not, perhaps, realised how greatly mathematical instruction has increased of late years in evening classes. In the first instance, there has been a growth of what is called the organised course, whereby a student, instead of taking a single subject in an evening course, such as machine construction, is now advised and encouraged to take an organised course which is particularly adapted to his occupation, and which generally includes a technological subject, a mathematical subject and drawing. The result of this has been to increase considerably the amount of mathematical instruction in evening classes. Secondly, the growth of the system of certificates awarded in conjunction by the Board of Education and by the professional institutions, such as the Institute of Mechanical Engineers, the Institute of Electrical Engineers and the Institute of Chemistry, has raised the standard of the work which has been done. As an illustration I might say that in one London Polytechnic alone there are 1100 evening students taking Practical Mathematics as a subject of their technical course, whilst in others there are 700 and 400 respectively. Probably the total number is double what it was some six or seven years ago.

Why do students need mathematics in their course? For two reasons: firstly, in order that they may understand the theory of their work in such

subjects as applied mechanics; secondly, that they may be able to make use of it in dealing with the formulae which they are constantly having to handle in engineering or building practice, chemical work, etc. Consequently, the idea is not to train professional mathematicians, but to train students to use their mathematics as a tool; and if so, they must use it correctly, otherwise, as a tool, it will be of very little value to them.

One may say that four-fifths of evening students have had no previous training in mathematics other than that obtained in the ordinary Elementary School. Possibly that may have been supplemented by a junior course, as described in the report, in an ordinary evening school before entrance into a Polytechnic. Then comes the three-year Senior Course which is set out in the report. It is obvious that if a student is to get through this course, and at the same time is to understand his mathematics in such a way that he can use it in engineering or building work, he must undertake a considerable amount of practice. Faulty technique is the chief weakness of the evening student in mathematics. I do not mean that he must necessarily do drill, although a fair amount is essential, but he must do a considerable amount of exercise work. I admit it is a hard life for the evening student: he has to attend three evenings of at least two hours each to get through the course and he has to do home-work, reading for other subjects, and exercise work. Nevertheless, the evening student does get through, and I think he deserves a great deal of credit for the way in which he does the work. Moreover, the amount of homework is increasing. Constant practice at his mathematical work is one of the foundations of any progress in mathematics of the evening student. That is why the report has laid considerable stress on that point. The technical student must be able to handle, transform and evaluate formulae, use logarithmic and other tables, and work out the result with a reasonable feeling that he is going to be accurate in his answer.

Mr. A. BUXTON (Cardiff Technical College): Mr. Chairman, I wish to say a few words with regard to the inclusion of theoretical geometry in the new syllabus for mathematics for the Junior Course in Technical Evening Schools. On p. 12 of the Report we read that "On the whole, the syllabuses proposed by us"—(i.e. the Sub-Committee)—"will be found to differ very little from those of well-known examining bodies. However, there is one point that is novel. After very careful discussion, we have formed the opinion that the introduction of a little deductive geometry would be desirable in the junior courses (ages 14-16)." The Sub-Committee had early to answer that important question, and it was helped to its conclusion by three prominent factors. In the first place, it decided that the object of the junior course was educational, so far as mathematics was concerned, rather than instructional. Secondly, there was to be no undue specialisation between the ages of 14-16; and thirdly, that the change in the treatment of theoretical geometry from the days when Perry formed Practical Mathematics classes all over the country to the scheme recently given in the Report on the Teaching of Geometry issued by this Association rendered the present time very opportune to improve the syllabus under discussion.

The task of creating the framework of the syllabus in deductive geometry was delegated to Mr. Tuckey and Mr. Hughes, and they drew up what appears in the Report under Appendix I. Part I. deals with "Fundamental Geometrical Ideas to be introduced by Drawing and other Experimental Methods." That corresponds really to Stage A in the recent report issued by this Association. Part II. deals with "Deductive Geometry." The present practice in Evening Technical Schools is that there is practically no deductive geometry done at all, and the object is to ensure that the boys shall work through the fundamental geometrical ideas and then go on to the deductive geometry. It is definitely stated that "An essential preliminary to this deductive work is a careful tabulation of the facts established (or accepted) in Part I., including

the properties of angles at a point, of parallel lines, of congruent triangles, and of similar triangles. It is important to remember that no attempt is to be made to prove these facts from Euclid's axioms or otherwise. They are to be arrived at by the informal methods suggested above, and then carefully stated and taken as assumptions on which the deductive work is to be based."

That, briefly, gives you an idea of the contents of the geometry scheme for the Junior Course. The theorems have been arranged in sequence, so that, as suggested in the *précis* report in your hands, the teacher does not confine his attention to one or two isolated theorems. The Report gives three Sequences—A, B and C; Sequence B being dependent upon Sequence A, whilst Sequence C is independent of Sequence A or Sequence B. So that, as well as improving the present arrangement, the theorems can be worked with some logical connection.

Professor T. P. NUNN: I have been asked to explain the main points of the method of teaching logarithms and exponentials which I have sketched in the Report. Logarithms may be taught either as a device for facilitating computation or as numbers exhibiting the properties of an interesting and important function. The method I recommend combines both points of view. The student of elementary mathematics—particularly, perhaps, the technical student—should become familiar with three functions of outstanding importance: namely those represented graphically by the straight line, the exponential curve and the sine curve. These may be regarded as symbolising three contrasted modes or laws of magnitude—change or growth; and the second, the one we are here concerned with, may be dealt with as follows.

Risking impropriety, let us imagine a small boy of (say) exactly 11 to be stripped and ringed with lines an inch apart. Also imagine a wave of a magic wand to convey him suddenly through time to his next birthday. Then the rings originally separated by an inch will now appear about 1.04 in. apart. If we wash him and repeat the operation, the rings an inch apart on his twelfth birthday will again become 1.04 in. apart by his thirteenth. If this mode of growth persists for some years, it is clear that the lad's height after 1, 2, 3, etc., years may be predicted by repeated multiplication of his present height by the "growth-factor" 1.04; also that his stature on previous birthdays can be recovered by repeated division of his present height by the growth-factor.

The graphic symbol presenting these facts will, of course, be a series of lines or ordinates erected at unit intervals along a time-axis, their heights increasing in geometric progression from left to right. Suppose we now ask what the height of our boy will be at a time between two birthdays. To answer such questions we may imagine ordinates to be interpolated between the original ones, the rule being always observed that the heights of any series of equidistant ordinates are in G.P. It is reasonable to expect that the ordinates of a smooth curve drawn through the summits of the original ordinates will have this property, and it is easy to test the validity of the expectation. Here is a "growth-curve" drawn for me some years ago by Miss Punnett, the growth-factor or "base" employed being 1.1; and here are some lithographed reductions of the figure, showing only the curve and the time-axis. These copies may be given out to a class; the students draw as they please pairs of equidistant ordinates, and verify that the ratio of the greater member of a pair to the lesser is constant for the same interval between them. This property of the curve is the foundation of all that follows, and must be well "driven home."

We now proceed to show that the curve can be used to solve certain arithmetical problems. For instance, to find the fifth power of 1.3 we start by locating the ordinate OY of unit height and the ordinate PA whose height is 1.3; and then draw the ordinate QB whose abscissa, OB , is five times the abscissa of PA , namely OA . Then, since QB is the fifth of a series of ordinates

at distances all equal to OA , we have $QB = (PA)^5$. By measurement, QB is approximately 3.7; hence $(1.3)^5 = 3.7$. Conversely, to find the fifth root of 3.7 we locate the ordinate QB whose height is 3.7, and find the ordinate PA whose abscissa OA is one-fifth of OB . Then the height of PA (1.3) must be the fifth root of 3.7.

Again, to determine by the curve the product of 1.3 and 3.7 we draw the ordinate RC whose abscissa $OC = OB + OA$. Then by the fundamental property of the curve $RC/QB = PA/YO = 1.3$; that is $RC = QB \times 1.3 = 3.7 \times 1.3$. Similarly, if RC is drawn so that the abscissa $OC = OB - OA$, then

$$QB/RC = PA/YO = 1.3;$$

that is $RC = 3.7/1.3$.

It will be seen that these results, which are easily generalised, contain all the ordinary rules for the use of logarithms. For if we give the name "logarithm" to the abscissa of any ordinate of this kind of growth-curve and name the ordinates by the numbers which measure their heights, then we have found that the logarithm of the n th power of a number is n times the logarithm of that number, the logarithm of the product of two numbers is the sum of the logarithms of the numbers, and so on. But before we proceed to introduce this terminology and to consider tables of logarithms, it is well to develop a little further the use of the curve.

Along the horizontal axis of the large drawing of the curve whose growth-factor or base is 1.1 you will observe a series of graduations. The numbers measure the magnitudes not of the abscissae, but of the ordinates above the points where they are inserted. The strip of paper containing these graduations may be cut away from the curve and used to perform the operations of finding powers and roots. For instance, to find the seventh root of 9.8 I lay the strip (we call it a "Gunter line" or simply a "Gunter") across a series of equidistant parallels, placing the beginning of the Gunter, graduated "1," on one of the parallels and swinging the strip round until the graduation 9.8 lies on the eighth of the series. It is evident that the parallels now divide the intervals between the graduations 1 and 9.8 into seven equal parts; so that, by the property of the curve, the graduation 1.4, where the second parallel meets the Gunter, must be the seventh root of 9.8. Multiplication and division can be performed by means of a Gunter line and a pair of dividers. For example, if the interval between the graduations 1 and 1.3 be taken between the points of the dividers and transferred beyond the graduation 3.7, the further point of the instrument falls upon the graduation 4.81, which must be the product of 1.3 and 3.7. But in performing multiplication and division it is much more convenient to substitute a second Gunter line for the dividers. In that way we reach the idea of a slide rule, and learn to use it before we have dealt with tables of logarithms or even formulated the definition of a logarithm.

Proceeding now to define logarithms and antilogarithms as the abscissae and ordinates of a growth-curve of the type studied, we observe that we can dispense altogether with the curve if only we make a sufficiently full table of its ordinates (numbers, antilogarithms) and the corresponding abscissae (logarithms). Tables of logarithms and antilogarithms will obviously differ according to the growth-factor or base of the curve; we particularise a table by naming the base, which is the number or antilogarithm whose logarithm is unity. All tables are equally legitimate, but it is easy to show that tables to base 10 are the most convenient for practical purposes. For let a curve be drawn with 10 as its base or growth-factor; then if any two ordinates are separated by unit distance, the greater must be ten times as long as the lesser. In such a curve the abscissa of the ordinate of height 3 will be found to be about .48; that is $\log_{10} 3 = .48$. Then, by the property of the curve, the ordinates whose heights are 30, 300, 3000, etc., have for their abscissae (logarithms) .48 + 1, .48 + 2, .48 + 3, etc.; similarly, the abscissae (logarithms) of

the ordinates whose heights are $\cdot 3$, $\cdot 03$, etc., must be $\cdot 48-1$, $\cdot 48-2$, etc. Thus if we make a table of logarithms of numbers between 1 and 10, the logarithms of all other numbers are obtained from these by the mere addition or subtraction of integers.

So far we have made no use whatever of the notion of indices, or indeed of any algebraic notation. In this respect we have followed the history of mathematics; for Napier invented logarithms and Briggs calculated his tables to base 10 many years before John Wallis, working on the foundation of the integral calculus, hit upon the idea of representing roots and the powers of roots by fractional indices. In the second, or advanced, course of technical mathematics fractional indices must be taught, and may with great advantage be defined in connexion with growth-curves. By the construction of the curve, the ordinates whose abscissae are 1, 2, 3, 4, etc., have the heights r , r^2 , r^3 , r^4 , etc., r being the base or growth-factor. Thus the formula $y=r^x$ gives the heights of these particular ordinates. It would obviously be convenient to extend the same formula to cover all ordinates of the curve; so that $r^{+2\cdot6}$, $r^{-3\cdot8}$, etc., would refer to the heights of the ordinates whose abscissae are $+2\cdot6$, $-3\cdot8$, etc. On this principle $r^0=1$; for the expression describes the height of the ordinate whose abscissa is zero. We can then define the expression r^n , for all values of n , as the antilogarithm of n to the base r , or the number whose logarithm is n in a table whose base is r . This definition is, I submit, much more sensible than the ordinary definition of a fractional index in terms of powers of roots. The ordinary definition does well enough if we consider simple instances such as $r^{\frac{1}{2}}$ or $r^{\frac{1}{3}}$. But the practical man has generally to deal with such expressions as $(1\cdot4)^{0\cdot3572}$; and it is much more natural to think of this as the antilogarithm of $0\cdot3572$ to base $1\cdot4$ than as the 10,000th root of the 3572nd power of $1\cdot4$.

A general definition of an index having been reached, we proceed to the discovery of the very important limit known as " e ," and to the differentiation of a^x and $\log_e x$. These investigations are all made extremely simple by means of the properties of the exponential curve. [The derivation of e , the proof that $L(1+x/n)^n=e^x$, and the deduction of the two differential coefficients from the exponential curve, occupied the rest of Prof. Nunn's speech, and are explained with sufficient fulness in the Report.]

Mr. W. G. BICKLEY: I was able to see the draft report prepared by the Sub-Committee some time since, and one point I noticed was that the Sub-Committee had not been able to find any completely satisfactory method of dealing with exponentials. Having treated that subject in a way of my own for some time, I wrote an article and sent it to the Editor of the *Gazette*. I believe it has been published in the January number. Dr. Piaggio asks me, not to give the article now, but the reasons which have led me to adopt my method. After some experience in secondary schools I took up the work of teaching mathematics to technical students, and I found the problem was very different. I had not realised till then how great the difference actually was. In the schools everything is more or less mapped out for us, but there the students have different aims. They come for instruction rather than for education, and it is our business to give that instruction in such a manner that the students can apply it to problems that arise in their technical studies. They seem, in fact, to have some idea themselves as to what they want; at any rate, a good many of them have an idea of what they *think* they want, but very few have any idea of pure mathematics. Provided we can show that pure mathematics is of use to them—and I think we must start from that standpoint—I do not agree that we have *merely* to instruct, but that our endeavours must be based, first of all, on giving sound instruction. That leads us to overhaul the subject-matter of what we are going to teach. It is no use teaching them things they are not going to use, for then, unfortunately, it is not easy to get them to listen. At any rate, we must try and show them first that whatever we do in pure

mathematics is useful and necessary. It seems to me the only way to do that is to start from a concrete practical problem. It does not matter how beautiful a picture of general theory you draw, you must have some nail upon which to hang it. Starting from the practical problem you do arouse the interest of the students, and it is then possible to carry on, on the lines of the article I have written, which I hope will be in your hands, sooner or later. Start from a concrete practical problem, give some idea of what the solution is and means before actually solving it, then deduce the formula which does satisfy the conditions. I disagree with one implication in the report: that it is possible to hurry along the applications too soon. I do not think you can bring in the practical applications too early. Moreover, it seems that is sound historically, because in so many cases it has been the technical need which has given rise to the mathematics.

Mr. J. KATZ (Croydon): I do not think I have anything to add to my article which, by a fortunate chance, happens to be printed in the December number of the *Mathematical Gazette*,—on how we are to introduce e into our teaching. I thoroughly agree with the whole spirit of Professor Nunn's method, yet, as regards teaching in Polytechnics, we are up against a practical difficulty, that the students know logs., or they think they do anyhow, long before they come to us. In a Polytechnic there are generally three or four people who have their hand in training these students. One man will take them for the first year, another for the second, and another for the third and pass them on to me for the fourth year. If I am to introduce them to e , I have no time to deal with the subject according to the method suggested by Professor Nunn. He told us that one difficulty was that the method required a lot of thorough teaching. There is not time for one man to do that; it might be possible if the same man took them in the first, second and third years. But if they come to you in the third or fourth year and you have not been responsible for the early teaching, I very much doubt whether you would be able to do that earlier work with the thoroughness required.

I have not seen the detailed report, but I am glad to notice the statement that there is no really satisfactory method of teaching " e " to people whose beginnings in mathematics are so weak. I still think that there is something to be said for my own way of teaching it, which has arisen out of reasons partly historical and partly practical. First of all there is the fact that the logarithmic function arose historically out of the failure to integrate x^a when $a = -1$. The difficulties arise because we have immature people with poor foundations who are tired at the end of the day, and we have to get through the work at a tremendous pace.

Dr. W. F. SHEPPARD: I should like to make a couple of remarks on the report. I take it that the object of advocating some deductive geometry is to enable the students to get a better grasp of geometrical relations and to link up their results. That, no doubt, is very important, but there may be a certain amount of opposition by those who read the report to the idea of deductive geometry. It occurs to me that in the report that particular aspect of the purpose of the thing is not sufficiently emphasised. Then there is the reference to the lack of practice in the elementary processes of algebra. I believe that applies not to the junior students, of whom there are comparatively few, but those who begin at about 16. They have been at public elementary schools, have left about 14, and had practically no algebra, and when they start at 16 they cannot perform the ordinary algebraic processes. There must be something seriously wrong if these students have got to undertake in the rather heavy strain of their work a good deal of algebraical manipulation. What is actually wrong I really do not know. I merely raise the point, because there does seem to me to be something wrong. I suppose it does not apply to those who have been through the junior course between 14 and 16. If that is so, have you considered those of 16? It seems to me that possibly the Committee has not

gone deeply enough into the point: that probably what is needed is some re-casting of the algebraical work.

Prof. PIAGGIO: The lack of practice in algebra applies to every stage, and it is very serious indeed. In fact, it is the most serious difficulty we have encountered.

Mr. C. O. TUCKEY: Mr. Chairman, Ladies and Gentlemen,—There is one small point in regard to geometry that it might be worth while to clear up. My function on the Sub-Committee was mainly to see that any recommendations made on the subject of geometry were consistent with previous recommendations made, I will not say by the Mathematical Association, but by the Teaching Committee which is being ejected from office in about 5 minutes. I hope that, even with the extraordinarily small amount of geometry which those who knew best the limitations imposed by time allowed us to insert, we have, at any rate, kept it in accordance with the previous reports. Those of you who have taken a term off and studied the M.A. Report on Geometry will have found on p. 17 that experimental work and class-room work are described in about half a page. That has formed the basis of what was also put in the preparatory schools report, which was slightly longer, mainly with the idea of helping classical masters who happen to be teaching mathematics. What we have in the present report is, again, substantially that experimental introduction with the barest minimum of the next stage, the deductive stage. As technical students are, perhaps, more concerned with experimental work, we put into the experimental work certain things which might possibly with public school boys be put into the deductive work. We put in under the heading of "Symmetry" the two fundamental locus theorems, and also the result about the tangent to the circle being at right-angles to the radius, the latter coming under the heading of the study of various symmetrical figures. Moreover, we have put in more with regard to similar triangles in the experimental stage than we did in the previous report. That is to say, there have been two slight additions to the experimental work. Then the deductive work is cut down to the barest minimum of theorems, the sum of the angles of a triangle, a group of theorems connected with the angles at the centre and circumference of the circle, and a group of the theorems on areas leading up to Pythagoras' theorem. We thought that would give some idea of deductive work, without overloading the two courses. I trust the members of the Association will agree that in this arrangement we have followed the spirit of the large report on geometry which was published previously. The whole thing is consistent, and the modifications made are those natural in the case of technical students as against those who have more time available.

The PRESIDENT: I think we can now close the discussion. Of course one realises that no elementary treatment of exponentials is likely to be what any sane person would call satisfactory. It is a question as to which is more illuminating or attractive than another method. I think Professor Nunn's method was very attractive. In some ways I almost wish I was sufficiently unsophisticated to believe it was as easy as it looked!

Mr. C. O. TUCKEY: I should like to propose a hearty vote of thanks to the President for his work as President, and, in particular, for his most attractive, vigorous and slashing address this afternoon.

The vote of thanks having been carried by acclamation, the proceedings terminated.

353. Capt. Blifil, in calculating the death of Mr. Allworthy, "had employed much of his own algebra, besides purchasing every book extant that treats of lives, reversions, etc. From all which he satisfied himself, that as he had every day a chance of this happening, so had he more than an even chance of its happening within a few years."—*Tom Jones*, II. viii.

REVIEWS.

SCHOOL GEOMETRIES IN GENERAL AND IN PARTICULAR.

(I.) **The New Matriculation Geometry.** By A. G. CRACKNELL and G. F. PERROTT. Pp. x + 303. 4s. 4d. 1925. (University Tutorial Press.)

(II.) **A School Geometry on "New Sequence" Lines.** By W. M. BAKER and A. A. BOURNE. Pp. viii + 307. 4s. 6d. Bks. I. to III., 2s. 6d.; Bks. I. to V., 4s. 1925. (G. Bell & Sons.)

A student of Geometry if he carries his studies far enough may expect to learn (i) that the proof of the three-sides case of congruence of triangles must follow the proof that the angles at the base of an isosceles triangle are equal since it depends on this result; (ii) the proof of Pythagoras' theorem. The writer of a Geometry for Schools should be quite clear in his own mind which of (i), (ii) should be learnt first.

The M.A. Report on Geometry, agreeing with the view expounded in Circulars 711 and 851 by the Board of Education, recommends that (ii) should be learnt before (i), since (ii) belongs to the Deductive and (i) to the Systematizing stage.

Most, however, of the text-books appearing with the magic words "New Sequence" made prominent take the other view. In this they appear to have no support from the A.M.A. Report itself, which in No. I. of its "conclusions and recommendations" states that "no formal proof should be required" of (among others) *Euc. I. 8*, the "truth of which should be established by intuition and experiment."

The trouble is that the A.M.A. Report, while definitely refusing to advocate a fixed sequence, did definitely publish a sequence (and I hope my presumption will be pardoned if I add an excellent one). It, however, omitted any very clear guidance as to whether this sequence was to be followed *ab initio* by beginners, or whether it was to be a summing up of geometrical knowledge to be tackled in the last year before matriculation by students who had already mastered all its facts and the most important of its proofs.

Thus there is a great temptation for the text-book writer to supply proofs for the propositions of the sequence with riders graduated from those suitable to extreme beginners placed among the early propositions to those of much greater difficulty placed with the results of *Euc. III.*, and not to realise that his book is unsuitable to the beginner of 12+, both because his early propositions are too hard and because his later riders are too hard, and that his book is unsuitable for revision at 15+ because his early riders are too easy.

To supply proofs in order to propositions in a definite sequence and to select, from the immense store which is available, exercises and riders to follow each proposition or to fit in where the pagination permits is the easy path which was open to the editor of Euclid for schools, but is not for the modern teacher who believes in a Deductive stage in Geometry, earlier than and distinct from the Systematizing stage.

The text-book writer with this belief can if he pleases make the bulk of his book of exercises, the exercises on each group of theorems being severely graduated with some suitable for beginners and some for comparative experts, and the chain of theorems being placed at the end where they can be used at the teacher's discretion; or he may give twice over the proofs of all those theorems which he includes in the deductive stage, not fitting them into their places in the logical chain until the second time.

There are well-known text-books arranged in both of these ways, of which Durell's and Godfrey and Siddons' may serve as examples. There seems also to be a third possible arrangement, similar to that of some of the concentric-system geographies, in which the whole is arranged in its final order and the stages are separated by devices of printing. This idea applied to geometry seems to present such difficulties that as far as I know it has not yet been tried.

The two books whose titles are given above ignore any distinction between the Deductive and Systematizing stages.

(I.) seems to me a book which does with success that at which it aims. It should be most suitable for a student who is *beginning* geometry at the age of *fifteen* with a view to matriculation. It is better adapted than most books for a student who has little help from a teacher. There is in particular an excellent chapter on "the solution of riders" (given as Chap. XVII., but for use earlier). Chapter XVI. on "similar figures" gives a suitable informal treatment to lead up to the proofs by similar triangles of the rectangle properties of a circle. Any formal treatment of similar figures is beyond the scope of the book, which contains the substance of Euc. I. to IV.

Note that, as explained above, I regard this book as quite unsuitable for a beginner of twelve or thirteen.

(II.) must, I think, be regarded as weak at the beginning. There is an introductory chapter on Experimental Geometry (pp. 1 to 17) which contains little that is not commonplace, and Book I. is not suitable for a beginner of about twelve.

On the other hand, the early part might well be used for revision at fifteen, and there is much good material in the later parts of the book. To suggest the amount of ground covered, Book IV. contains "Radical Axis" and Book V. Ptolemy's theorem and also alternative proofs from Euclid's definition of proportion for the fundamental propositions of Euc. VI.

An appendix to Book V. deals with Inverse Points, Pole and Polar, etc., though briefly and with few exercises.

Book VI. deals with the Geometry of Space, and Book VII. with Solid Figures, in an attractive manner.

On the whole this might well be used as a second Geometry, especially for those who are giving a good deal beyond matriculation standard.

C. O. TUCKEY.

Vectorial Mechanics. By L. SILBERSTEIN. 2nd edition. Pp. x+205. 10s. 1926. (Macmillan.)

It is a great pleasure to welcome this book on its return into print. No one familiar with the literature of the subject needs to be told that Dr. Silberstein's is beyond comparison the best book in the language for the reader who wants to understand the vectorial outlook on applied mathematics, and that far from being merely this, it is also, in the author's words, "an almost systematic exposition" in 118 pages "of the chief parts of mechanics". The first chapter is a short, clear, and logical account of vector algebra and analysis, from the primitive notions to the properties of the Hamiltonian operator ∇ . The exposition of mechanics leads from d'Alembert's principle to Hugoniot's theorem on second-order discontinuities in hydrodynamics, dealing on the way with such matters as Lagrange's equations and Hamilton's principle, motion under no forces, strain and waves in elastic solids, and vortex motion.

This second edition is a line-for-line reprint of the first, supplemented by a few notes, of which the longest refer to the system of projective vector algebra which Dr. Silberstein developed in a booklet published in 1919, to the linear-vector operator treated otherwise than in the text, and to the Eulerian angles for the orientation of one trirectangular frame with reference to another. These notes are not of such value that anyone who already has the book need indulge in the new edition, but every young student of mathematics and every teacher of mechanics to whom the book is unknown should seize at once the opportunity to make its acquaintance.

E. H. N.

Differential Equations. By H. B. PHILLIPS. 2nd ed. Pp. vi+116. 6s. 6d. net. 1924. (Chapman and Hall.)

The first edition of this book was reviewed in the *Gazette* for December, 1922. The new edition is about half as long again as the first, which was certainly much too short. The principal change is in the opening chapter, which now includes an account of geometrical problems leading to differential equations. The number of problems has been considerably increased, and a large proportion relate to mechanics and physics. The second edition is a distinct improvement on the first, and the book will appeal to those who are interested in differential equations chiefly for the sake of their applications.

H. T. H. P.

Plane Curves of the Third Order. By HENRY SEELAG WHITE. Pp. xii + 168. 12s. 6d. net. 1925. (Cambridge, Mass., Harvard University Press; London, Milford, Oxford University Press.)

Many text-books have dealt with plane cubics; it was hardly to be expected that we should find anything new in the present work except novelty of treatment and a fresh point of view. The author has not attempted to give a complete account of the properties of cubic curves. Metrical results are rare, and foci are not even defined. No mention is made of the interesting theorems connected with circular cubics. The usual discussion of nodal and cuspidal cubics by aid of an algebraic or trigonometric parameter is absent. Hence the author confines himself almost entirely to the projective properties of non-singular cubics. Here he covers the ground with reasonable completeness, though he limits himself to the Weierstrass elliptic function when expressing the coordinates of any point parametrically; and it is to be regretted also that the chapter on apolarity was not made more attractive by the inclusion of some of the elegant geometrical results due to Milne and others.

The methods of pure and analytical geometry are skilfully blended, while the author takes much care to make his proofs rigorous. His appeal will be to the mathematical expert rather than to the younger student. There are no examples for exercise (except those on p. 122); and the novelty of the elegant, but highly condensed, symbolic notation will prove a stumbling-block to a reader who is not used to it in works on the theory of quantics.

The book is clearly printed and illustrated, while misprints are rare.

Curve Schembe Speciali Algebriche e Trascendenti. Volume secondo. By GINO LORIA. Pp. 255. L. 50. 1925. (Bologna: Nicola Zanichelli.)

This second volume contains Chapters 9 to 12 of Prof. Loria's treatise. The book contains a mass of information about various twisted curves, which succeed each other with comparatively slight logical sequence. Their properties are proved for the most part, mainly by methods of analysis rather than pure geometry, though in a few cases the author contents himself with mere enunciation. The style is clear and pleasant, while the printing and arrangement are good. The book makes easy reading. The treatment of lines of curvature, geodesics, Darboux lines, and asymptotic lines is rather slight; but helices, curves on a sphere, curves whose tangents belong to a linear complex, Bertrand and allied curves, loxodromes, etc., are given a fairly full discussion.

A student wanting examples for practice on the theory of twisted curves would gain much benefit by taking a few of the interesting properties here enunciated, and supplying proofs for himself. But, apart from this, the work forms a useful book of reference.

HAROLD HILTON.

(1) **Intermediate Light.** By R. A. HOUSTOUN. Pp. 228. 1 coloured plate, 162 figures. 6s. 1925. (Longmans, Green & Co.)

(2) **A Treatise on Light.** By R. A. HOUSTOUN. Pp. 486. 2 coloured plates, 334 diagrams. 12s. 6d. 1925. (Longmans, Green & Co.)

(1) This book would seem to be singularly well adapted to the purpose for which it is written. It is clear and concise, and the chapter on the mixing of colours is particularly simple and illuminating. The paragraph at the end on the Quantum and the Einstein Star Shift is evidently written by one holding the more conservative views. The last sentence, "It is very difficult to fit this result in with our previous ideas on the subject" might have been amended so far as to mention that Einstein's theory assumes a change in our views as to what constitutes a straight line. Nevertheless, the author must be thanked for having mentioned these two theories so that the student may realise that physical knowledge is still subject to change. This will help him to maintain a healthy scepticism. On the whole the book is to be highly commended.

(2) A book of this character suffers from certain inherent defects. Its necessary comprehensiveness must militate against a sufficient slowness of development in the more difficult parts of the subject. The chapter on the electro-magnetic theory of light and the following chapters would require

considerable expansion at the hands of the teacher to make the student fully conversant with the subjects handled. But this is inevitable.

In Chapter XXI. the author shows that the orthodox wave theory permits of light being propagated in pulses, and he shows that such pulses may produce dispersion and interference. If the Quantum hypothesis were only applicable to a small body of facts this work might have some permanent value; but it is scarcely to be doubted that there will be a complete revolution in our attitude towards waves and geometry in the next decade or so. In the light of these probabilities this chapter would seem out of place in a student's text-book, interesting as it is from a logical point of view. In any case, the hydrodynamical analogue presented is far too abstruse to serve as an elementary introduction to the subject of pulses. It is surely much better to use such an illustration as an explosion wave, and develop the matter as far as possible without the integral calculus. At this stage the average student will not appreciate the point of the mathematics which is really to be found in the Fourier Integral and Riemann's theory of characteristics.

On the whole the book is as satisfactory as one can expect to be written for the purpose required, and the attention paid to geometrical optics is specially to be commended. But the book would have been improved by the omission of the brief and rather misleading paragraphs near the end on the Principle of Relativity and the Quantum Theory. To say "But the Michelson-Morley experiment is a somewhat narrow experimental basis on which to rear such a structure" in a book published in 1925 is scarcely fair.

The University, Sheffield.

P. J. DANIELL.

1. **Mathematics of Life Insurance.** By L. W. DOWLING. Pp. x + 121. 8s. 9d. (McGraw Hill Publishing Co. Ltd.)

2. **Mathematics of Finance.** By LL. L. SMAIL. Pp. xv + 310. 15s. (Same publishers.)

These books are of the elementary kind; the first is intended "as a course for those young men and women who wish to become trained actuaries; or as a final course" for certain other students. The author appreciates that, though his book assumes only an elementary knowledge of algebra, a considerably greater mathematical equipment is essential to an actuary, and we think it was a mistake to assume that the mathematics were not available for the first course. In England the mathematical work comes first and the study of life contingencies afterwards; and this arrangement seems the right one. If, however, an author is determined to assume only elementary knowledge of algebra it is inconsistent to write a concluding chapter on the normal curve, Stirling's formula, standard deviations, etc., in which integrals are used (sometimes where they might have been avoided), or to define Makeham's "Law of Mortality" in terms of a function (force of mortality) which involves a differential equation, when it can be defined just as easily in terms of a simpler function (the logarithm of the probability of surviving a year).

The other book is longer, and deals largely with interest, bonds, etc.; it then discusses in elementary fashion for twenty-five pages some of the easiest parts of life contingencies, and finally gives an unnecessary list of formulae and some tables which are so easily accessible elsewhere that no useful purpose seems to be served by incurring the expense of re-printing them. The book assumes that the student using it has a "minimum preparation in Algebra"; he, in fact, has to have the sum of an arithmetical progression explained. To call such a book the *Mathematics of Finance* is, to say the least, an exaggeration! If it is necessary to provide for such students they should be given an arithmetical book without any algebra. The author might have produced such a book, as much of the arithmetical work on interest is done carefully and clearly, though at considerable length. On page 5 it is stated that "ordinary" interest is calculated on a basis of 360 days in a year: if the statement is correct as regards American practice we hope it will not spread.

We are afraid that we cannot see why books of this kind are required; the field is already covered sufficiently, and the justification for a new book rests, therefore, in some originality of arrangement, treatment, or outlook—a quality which is absent from the volumes before us. W. PALIN ELDERTON.

Elementarmathematik vom höheren Standpunkt aus. Band II. Geometrie. By F. KLEIN. 3 Aufl., edited by E. HELLINGER and FR. SEYFARTH. Pp. 302. 15 Goldmk. 1925. (Springer, Berlin.)

This second volume continues the survey into the region of geometry, but with an altered plan. The author sets out to give an encyclopedic review of the whole subject, which cannot be done in a short readable book like this. He gives us instead a most interesting account of his own preferences and points of view. Geometry is treated as the invariant theory of a group of transformations of coordinates suggested by physical movements. Inversion is mentioned (the word *Inversion* is unfortunately used for reflexion in the origin); but its nature is concealed by the artifice of taking the line at infinity as a point, so that it does not lead on to higher transformations, which are ignored except for a few remarks on maps. Room is found for reciprocation, vector analysis, non-Euclidean geometry, and a long criticism of Euclid. *Fusion* is prominent, signifying the early introduction of three dimensions. There is practically no pure or differential geometry.

About a quarter of the book is devoted to an account of the teaching of geometry in England, France, Italy and Germany. In our own country, the absence of a rigid government curriculum allows a foreigner only to notice new tendencies at haphazard, and a very odd selection of our text-books is mentioned, mainly those which have been translated into German. The A.I.G.T. effected "tame reforms," and Perry gets the credit of whatever progress our conservative system has shown. On the continent, reform can only mean a new syllabus, and is often a matter of politics rather than pedagogy. An appendix by Seyfarth brings this section up to date. H. P. H.

Experimental Optics: A Manual for the Laboratory. By Dr. G. F. C. SEARLE. Pp. xvi + 357. 16s. net. (Cambridge University Press.)

Dr. G. F. C. Searle's powers as an original teacher of Experimental Physics are known to many Cambridge men by personal experience, and to a still wider circle through his books on Experimental Elasticity and Experimental Harmonic Motion, which have deservedly found their place in most Physics laboratories. All these will welcome the news that the third volume of this series, *Experimental Optics*, which has been long expected, has now appeared. To have excited high expectations is not invariably an asset to a new publication, but it may be said at once that in the present case there are no disappointments in store for the reader.

The present volume contains accounts of the experiments which make up the course in Practical Optics in Dr. Searle's practical class at the Cavendish Laboratory. Most of them are well worth inclusion in any optical course, and not a few are highly original. Dr. Searle shows great ingenuity in translating the results of mathematical analysis into experimental form, and he has found in optics a very fertile and congenial field for his imagination. Each experiment is fully and accurately described, so that no reader should find difficulty in following out the course of the experiment even in its smaller details, and practical examples appended to each experiment illustrate the degree of accuracy which a careful student may reasonably expect to attain. The apparatus required is generally neither complicated nor costly, and most of it could easily be constructed in any workshop from the very adequate descriptions given in the book.

As a laboratory manual the book is as practical as it is original. Each experiment has been tried out by successive generations of students, and has gradually ripened to its present state of perfection. Any teacher who wishes, as many no doubt will, to introduce any of these experiments into his own courses may be assured that he will find them thoroughly sound and workable. This book should certainly find a place, by the side of its predecessors, in every Physics laboratory.

It is, however, something more than a mere manual for the laboratory. Since each experiment is accompanied by a full mathematical treatment of the parts of the subject involved, we have as a result a fairly complete textbook on Geometrical Optics. Physical Optics, which is dealt with only in

the last two chapters, is not so completely covered. The treatment of the subject is simple, and mainly geometrical. Some of the proofs are thus a little long. On the other hand, the attention of the student is focussed throughout on the physical phenomena involved in a way which would hardly be possible in the case of a higher and more elegant analysis. Teachers of Optics, whether their bias is, or is not, towards the experimental side, will find in this very practical volume valuable hints in the art of presenting the subject to "pass" students.

The exposition throughout is clear, and the book may confidently be recommended to university students of the subject. By publishing these scholarly and original manuals Dr. Searle is doing real service to science, and we hope that we may have still further volumes from him in the not too distant future.

J. A. CROWTHER.

Engineering Applications of Mathematics. By W. G. BICKLEY, M.Sc. Pp. vi + 188. 5s. net. 1925. (Pitman.)

This book is an admirable collection of over 400 problems in engineering and allied subjects, divided up into types, each type being illustrated by one or more worked examples, with a few general remarks on the methods used. The standard of mathematical knowledge required for the solution of a large proportion of the problems which arise in engineering and parts of physics is not very high, but the average student is generally by no means an expert in mathematical calculation, and his chief difficulty is the translation of the problem into mathematical symbols. He requires a good deal of practice in making up his equations from the practical data, and there is great need of a book of this kind, which will serve as a mine for the student to delve in. The sets of problems are not mere repetitions of one example with different figures involved, but they are classified according to the kind of equation they produce.

The book begins with a chapter on problems requiring mere differentiation, and follows this naturally with those problems in which the rate of change is known, i.e. problems on, as the writer calls it, "inverse rate of change." Then follows true integration, or "adding up," and the fourth chapter brings in differential equations.

All the well-known problems appear, e.g. friction of belts, deflection of beams, whirling of shafts, etc.; and there are numerous questions on general dynamics, electric vibrations and thermodynamics.

Mr. Bickley has produced a book which should be welcome to all teachers of engineering.

W. M. R.

Outlines of Mechanics. By A. H. E. NORRIS, B.Sc. Pp. 264 + xiii. 5s. net. 1925. (Mills & Boon.)

As we find the unit of mass chosen sixteen pages before the discussion and definitions of mass and inertia, and as the treatment of work and energy seems to us to be ill-arranged, we cannot agree with the author that he has had an unusual regard for order and logical sequence.

The fact that axioms are stated does not make a book logical; that the axioms are not explicitly referred to is rather an indication to the contrary; and that some axioms should be deduced from others merely shows that the author does not understand what an axiom is. Another bad feature of the book is a slipshod style. For example, we are told that "The range of the work is comprehensive, and indicates the relations between the several parts," and that "The foot is one third of a certain rod." The treatment of (statical) friction is most unsatisfactory.

An unusual feature is the attempt to indicate the points of similarity and of difference between the Theory of Relativity and Newtonian mechanics. It is doubtful whether very much can usefully be said about this, even to the student who promises to become a first-rate mathematician, when he is starting mechanics. Certainly the third-rate physicist, who is content to learn his mechanics from a book of this kind, had better give up any idea of understanding the Theory of Relativity from the outset.

A. R.

THE LIBRARY.

160 CASTLE HILL, READING.

ADDITIONS.

The Librarian reports the following gifts :

From **Mr. J. Brill** :

Offprints of two papers, and

A. OPFERMANN *Quadrilatère Complet* - - - - - 1919From **Mr. F. J. Cock** :L. BACHELIER *Calcul des Probabilités ; I* - - - - - 1912

C. W. C. BARLOW and G. H. BRYAN

Elementary Mathematical Astronomy (2 (1893) rep.) - 1903

J. BAUSCHINGER and J. PETERS

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A. DE MORGAN *Budget of Paradoxes* - - - - - 1872*Formal Logic* - - - - - 1847L. E. DICKSON *History of the Theory of Numbers ; I : Divisibility and Primality ; II : Diophantine Analysis* 1919, 1920J. DODSON *The Mathematical Repository* (3 vols.) - 1748, 1753, 1755
*Solutions of problems in algebra and in the theory of chances and annuities.*A. S. EDDINGTON *Space Time and Gravitation* (2 (1920) rep.) - - - 1921T. GALLOWAY *Probability* - - - - - 1839*The article in the seventh edition of the *Encyclopædia Britannica* reissued in book form.*J. W. L. GLAISHER
Factor Table for the Fourth Million - - - - - 1879*Factor Table for the Fifth Million* - - - - - 1880*Factor Table for the Sixth Million* - - - - - 1883*These tables were constructed to fill the gap between Burkhart's and Dase's. Has any friend of the Association these companion volumes to give ?*J. F. W. HERSCHEL
Examples in Finite Differences - - - - - 1820*C. Babbage's *Examples in Functional Equations* is as usual included.*L. B. W. JOLLEY *Summation of Series* - - - - - 1925*A large collection of results with references.*J. M. KEYNES *Probability* - - - - - 1921

D. N. LEHMER	Factor Table for the First Ten Millions - - -	1900
	List of Prime Numbers from 1 to 10,006,721 - -	1914
K. PEARSON	The Chances of Death, and other Studies in Evolution (2 vols.) - - -	1897
	Includes four statistical essays, one of which is an analysis of Monte Carlo roulette records.	
J. H. POINCARÉ	Calcul des Probabilités - - - - -	1896
E. E. SCOTT	Logarithms and Anti-Logarithms to ten places, with a Table of Constants - - - -	1897
I. TODHUNTER	History of the Theory of Probability - - -	1865
G. U. YULE	Introduction to the Theory of Statistics - -	1911
	Bibliotheca Chemico-Mathematica (2 vols.) - - -	1921
	Compiled and annotated by H. Zeitlinger and H. C. Sotheman.	
	Decimal Quotients for Vulgar Fractions; I - - - -	1823
	ALL PUBLISHED. Computed by Henry Goodwyn, a Smithfield brewer. "Whether the work shall be completed will depend on the reception which this...[Part] may meet with from the public." The workmanship and printing are admirable, but encouragement seems to have been lacking.	

Since the Library already contained works by Bertrand, De Morgan, Laplace, Quetelet, and Venn on the subject, as well as a complete edition of Pascal, a good collection of English and French writings on probability is now at the service of members, and it is the more to be hoped that someone will soon add at least Czuber's work.

WANTED.

Gazette No. 159 (July, 1922). The Librarian will repurchase clean copies at the price of 2s. 6d. from any members willing to sell.

Donations of back numbers of the "*Gazette*" are always welcome.

NOTICES.

The Edinburgh Mathematical Society proposes to hold a Summer School at St. Andrews, from August 3rd to August 13th, 1926. Courses of lectures will be given by Professor Birkhoff of Harvard, U.S.A.; by Dr. Richmond of King's College, Cambridge; by Professor Brodetsky of Leeds University; by Professor Gibson of Glasgow University; and by Professor Turnbull of St. Andrews University.

THE JOURNAL OF THE LONDON MATHEMATICAL SOCIETY.

The first number of the new *Journal*, which is issued by the London Mathematical Society in addition to its *Proceedings*, consists of 64 pp., and contains notes or papers by Messrs. Hilton, James, W. P. Milne, Pólya, Fekete, Hardy and Littlewood, Dalzell, Ingham, Titchmarsh, Landau, and E. A. Milne. An obituary notice of Felix Klein is contributed by Prof. H. F. Baker. "Notes on the Early History of the Society" is Dr. Glaisher's address at the Sixtieth Anniversary Meeting, and is full of attractive reminiscences of bygone days. The new *Journal* is to be published quarterly for the Society by Mr. Francis Hodgson, free to members, and 20s. per annum to the general public.

ERRATA.

- ix. p. 130, Footnote. For Schorten's read Schooten's.
- xii. p. 22, Note 695, l. 1. For C^1 read L^1 .
- xii. p. 61, Note 698, l. 1. For δ read R . δ a. β .
- xii. p. 111, Note 711, l. 1. For C^1 16.5 read L^1 16. b."
- xii. p. 170, l. 3, par. 4. Replace comma by full stop.
- xii. p. 510, l. 8 up. For $\angle w$ read Lw .

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